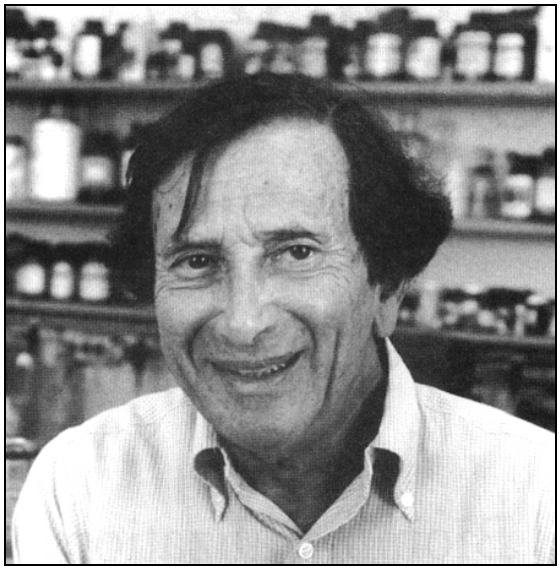

HEINZ A. LOWENSTAM
(1912-1993)

INTERVIEWED BY
Heidi Aspaturian

June 21-August 2, 1988

ARCHIVES
CALIFORNIA INSTITUTE OF TECHNOLOGY
Pasadena, California

Subject area

Geology, geobiology

Abstract

Interview conducted in eight sessions in the summer of 1988 with Heinz A. Lowenstam, professor of paleoecology. Dr. Lowenstam was born in Germany and educated at the universities of Frankfurt and Munich. He emigrated to the United States in 1937 to continue his graduate studies in geology and paleontology at the University of Chicago, receiving the PhD there in 1939. After a stint at the Illinois State Museum, he joined the Chicago faculty in 1948, working with Harold C. Urey on paleotemperatures. He joined Caltech's Geology Division in 1952 as a professor of paleoecology, pursuing research in a variety of fields. In 1962, he identified iron in chiton teeth, the first known instance of biomineralization, later found in such diverse creatures as bacteria, honeybees, and birds. In this interview, he recalls the difficulties he faced as a Jew in Nazi Germany, his graduate work in Palestine in the mid-1930s, his life as an émigré, his investigation of Silurian fossils in the Chicago area, and his interaction with such mentors and colleagues at Chicago as Urey, N. L. Bowen, Bailey Willis, Bryan Patterson, and Karl Schmidt. He discusses the evolution of the Geology Division at Caltech; its important move, under division chairman Robert P. Sharp, into geochemistry in the early 1950s; his work on the

paleoecology of marine organisms; his recollections of Caltech colleagues, including Sam Epstein, Beno Gutenberg, Hugo Benioff, James Westphal, Max Delbruck, and George Rossman; and the changes that took place in the division over the decades since his arrival.

Administrative information

Access

The interview is unrestricted.

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Preferred citation

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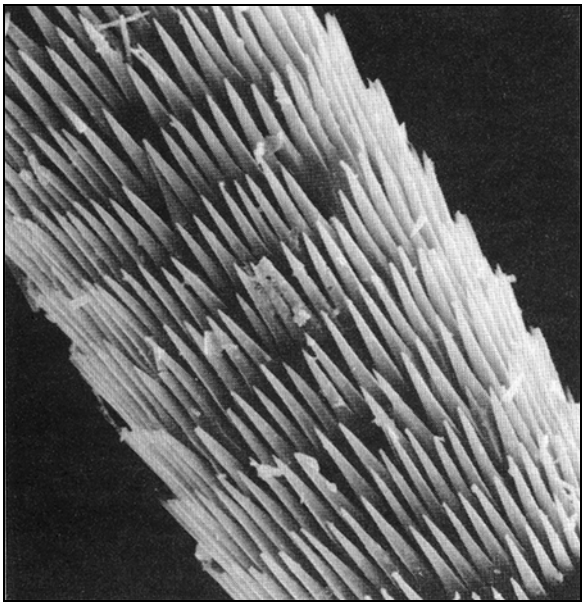
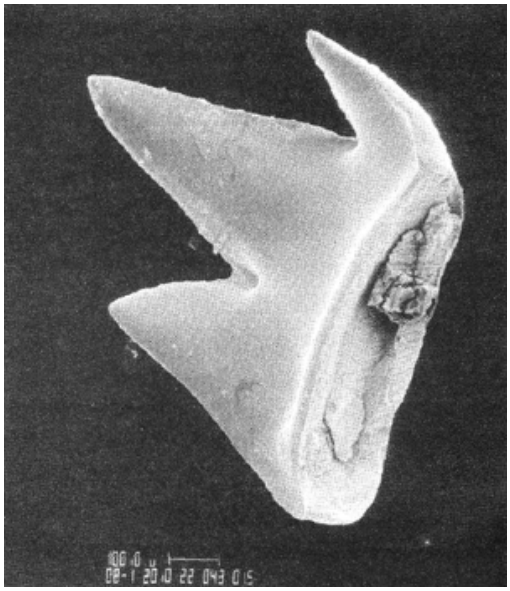
Lowenstam holds a Strombus shell in his aquarium room at Caltech, 1950s.



Scanning electron micrograph of a tunicate (*Herdmania momus*) spicule composed of vaterite that Lowenstam studied in the 1970s and 1980s.



Scanning electron micrograph of a single chiton (*Cryptochiton stelleri*) tooth showing its magnetite outer coating.

All photos from the scientific archive of Heinz Lowenstam housed at the Weizmann Institute of Science (Steve Weiner). Used by permission.

CALIFORNIA INSTITUTE OF TECHNOLOGY

ORAL HISTORY PROJECT

INTERVIEW WITH HEINZ A. LOWENSTAM

BY HEIDI ASPATURIAN

PASADENA, CALIFORNIA

Caltech Archives, 1991

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Heinz A. Lowenstam **by Heidi Aspaturian**

Pasadena, California

Session 1	June 21, 1988
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ASPATURIAN: Let's begin with your early background—where you were born, and a little bit about your parents.

LOWENSTAM: I was born in 1912 in Upper Silesia, in what was then southeastern Germany and is now south-central Poland. The place was Siemianowitz, which was a suburb of Laurahütte—a mining district with a steel mill. It was a horrible region, actually; it was like Dante's *Inferno*. Across the whole horizon, you saw belching chimneys spewing out fumes from lead smelters, zinc smelters, and steel mills. There were coal mines and iron foundries. The air was so poor that our plants in the house had to be specially tended so they didn't die from the fumes. In those days, there were no pollution controls. That was one reason why, as soon as I could get around in high school, I would get out on my bicycle and collect fossils and do other things—to go as far away as I could to see some nature.

I would like to talk first about my mother. She was unique. Before World War I—before she was married—she was art editor of a newspaper. She also wrote poetry; she wrote it until

she died. She was known privately in circles, although she never published anything. People would come and listen to her readings of her poems. I remember being once in Rio de Janeiro, where they went in the Nazi period, and there was a whole group of people sitting there listening to her reading her poetry.

My mother was very much interested in nature and got me interested in that, first by taking me around, showing me things, and then by getting me semi-popular publications on natural history. She didn't have any intention of having me go into anything like that; it was just to be educated.

ASPATURIAN: Were you an only child?

LOWENSTAM: No, I have a sister, now in Brazil, who is two years younger than I.
My father was also unique, in the sense that he was a classicist. He went to the *Gymnasium*—the German academic high school. He always had pockets full of books. He was more interested in history and literature. But he had to make a living, so he had to do other things. He was originally in a big lumber company. His function was to go to Russia—this was before the Bolshevik Revolution—and mark trees that would be cut for lumber, to be sent to Germany. After the lumber company, he started a furniture business and made furniture. That was until World War II broke out and they left Germany for Brazil. Then, somehow, my sister and I could support him sufficiently so he could do what he wanted to do, and he got interested in the history of the Jews of Brazil and wrote a two-volume sequence on that. He wrote it in German, and then one of his friends translated it, and it was published in Portuguese.

ASPATURIAN: Did either of your parents have a university education?

LOWENSTAM: No. But my mother was interested, among other things, in ancient Egypt, and she taught herself to read hieroglyphics. We would go to museums, like in Berlin, and she would read the inscriptions just like that and translate them.

ASPATURIAN: What about your early education?

LOWENSTAM: I went to elementary school four years, and then to *Hochschule*—high school. That meant math, physics, and chemistry, and modern languages rather than Greek and Latin, like my father and my grandfather had done, because I was considered too dumb to go to the *Gymnasium*. [Laughter]

ASPATURIAN: How did they make that kind of judgment?

LOWENSTAM: I don't know. [Laughter] But they decided it was better. They considered the *Hochschule* education to be more like a trade school. High school in Germany lasted nine years, and included what we would call the first two years of junior college. There was the *Gymnasium*, which taught Greek, Latin, history, and literature as major subjects. Math, physics, and chemistry were de-emphasized there, whereas the school I went to emphasized math, physics, and chemistry, English and French—just more like a trade school. Which was very fortunate for me. If you graduated from high school, then you were entitled to go to any university. Without it you couldn't go.

ASPATURIAN: Was your family religious? Did you come from a religious environment?

LOWENSTAM: First, I should qualify the word "religious." My father belonged to the Jewish Reform movement, but we had a kosher household, and my father was very much concerned about it. He had a brother in Spandau who was a Reform rabbi. My father belonged to various Jewish organizations, so I was brought up in that environment. I couldn't find it interesting enough; it was boring to me, these Talmudic arguments. Yet I felt I had no right to abandon them until I knew a little more about it, so I dragged through quite a bit of it. When I went to the University of Frankfurt, I also went to the yeshiva there, and after about six weeks I thought I had the answer: I knew I didn't want any part of it, so I gave it up.

ASPATURIAN: Did you and your family consider yourselves Jewish, as opposed to being German?

LOWENSTAM: My father considered himself a German of Jewish religion. When he was a young

man, he was a Zionist, but later he became more German. My mother never expressed any political views. I was very attached to the area, because of the whole history, but I was always in opposition to organized religious thinking. I was a black sheep all the way.

ASPATURIAN: In your family? In what ways?

LOWENSTAM: Starting with being interested in fossils, an interest that was incomprehensible to everybody.

ASPATURIAN: But your mother encouraged it.

LOWENSTAM: Encouraged the biologic part. She went along, but she didn't know much about it. In society, most of my friends were non-Jewish, except my closest friend. I was attached to the environment in which I grew up, but as I grew older and saw what was going on in Germany, I became very disturbed.

ASPATURIAN: You described the town you grew up in as very heavily industrialized. What were some of the strongest influences on you while you were growing up there?

LOWENSTAM: As a kid, I played on a mine dump—you know, the stuff that goes out from a lead and zinc mine. I wasn't supposed to go there, but I went with some miners' kids, and we played. We normally picked up a rock to throw, and one day the one that I picked up was awfully heavy. I knew it couldn't be an ordinary rock, so I broke it. It looked like silver. It was galena—lead ore. And that's what started me initially to collect minerals.

ASPATURIAN: How old were you when this happened?

LOWENSTAM: In elementary school. I'd collect all kinds of minerals. My grandfather and uncle—his son—were quite interested seeing me do things like that. My grandfather was a very famous interior decorator.

ASPATURIAN: Your mother's father?

Lowenstem: Her stepfather. He decorated all the castles in our area. We had about two dozen dukes and earls there. He designed things. Everything was done in his workroom and then taken to the castles. My uncle—his son—went to the Bauhaus as a result of that. He was one of the early modern designers, who later went to Uruguay and started decorating the Gold Coast mansions of the millionaires at Punta del Estes. He was also very interested in my collecting. He knew a lot of the high-up people in the mining business. I remember one birthday, when I was just in the early days of high school, he brought me this huge box specially made with a glass lid, and it contained the different minerals that were mined or found in the area. So he helped me in this respect to get more interested.

ASPATURIAN: You said your mother also played a role in this?

LOWENSTAM: Oh, yes. I liked to bring animals home—frogs, the usual—and she encouraged that. I had been collecting minerals about three or four years when I decided at a certain stage in high school, and I don't know why, to change from minerals to fossils.

ASPATURIAN: In terms of what you were collecting as a hobby?

LOWENSTAM: Yes. I started to find fossils in the mine dumps, and I remember, there was a teacher from another school who had a fossil collection and wanted to become a mineralogist, so we exchanged collections, and then I really started systematically to collect. My chemistry teacher encouraged me. In Germany, the teachers and students in high school were quite apart. Yet he was interested enough to take me to the teacher's library, where they had books on fossils. Nobody had ever read them; the pages were uncut. But there was a whole series, and he let me browse through them and borrow whatever I wanted for a while.

ASPATURIAN: Was it unusual for a teacher to get this close to a student?

LOWENSTAM: Yes.

ASPATURIAN: Most of your other teachers were more distant?

LOWENSTAM: Very much so.

ASPATURIAN: You were six when World War I ended. What do you remember about that period and a little bit following?

LOWENSTAM: I remember it very clearly, because we starved. I still have the complex today. I have to control myself, when I go somewhere to eat, because I'm always subconsciously anxious to get as much as I can. [Laughter]

ASPATURIAN: Your father had no work after the war?

LOWENSTAM: My father had been taken into the army. He wasn't actually involved in the war, because he had a captain over him who was a very devout Catholic, and my father was very religious as a Jew. They got along very well. The captain looked after my father. Whenever they came around to round up more bodies for the front, he would know about it beforehand and would send my father off on some errand so that he wouldn't be around. So my father never entered actually combat, but he wasn't at home either. My mother by that time had taken over the furniture business, and all she could do was run it. She did very well, and my sister and I were living with the maid. The rationing was such that we got some kind of cereal, very coarse, and some synthetic, sweet sauce, pink, and that's what you got three times a day in very small quantities.

ASPATURIAN: How long did this last?

LOWENSTAM: Three years.

ASPATURIAN: What was your family's situation immediately after the war?

LOWENSTAM: When my father came back, he started to work again in the furniture business. It went quite well for a while. My mother always helped with the bookkeeping.

ASPATURIAN: Did the Depression of the twenties have much of an effect on your family?

LOWENSTAM: It wiped us out completely.

ASPATURIAN: What happened?

LOWENSTAM: My father was in business, so when inflation came, his customers just left. He became a representative for three of the major furniture companies. And as far as I know, until he left Germany, that's what he did. He did very well. But he had to start from scratch. Just to give you an idea of how things were, one day I went to see my grandfather—the interior decorator—in his office when he was sitting over the books, checking something. A very big bank note fell down from the desk, and I went over, picked it up, and gave it to him. He looked at me and said, "Well, boy, you found it, you keep it." I was all excited; it was something like the equivalent of fifty dollars today. I tried to think what I would do with it. And by the time I had decided, in less than a week, it wasn't worth a nickel. That's how the inflation went.

ASPATURIAN: How were the people you knew—your friends, your relatives, your community—affected during this period?

LOWENSTAM: Basically, with a few exceptions like my grandfather, who was still working with the nobility—they always had money somehow—people were very restricted in what they could do for a long time, and never really pulled fully out of it. I think that had something to do with Hitler coming to power, too. So many people were terribly affected by the Depression, and they blamed the Weimar Republic.

ASPATURIAN: What impact did this have on your education? Was there any effect?

LOWENSTAM: Yes. In a sense, it was very hard. You see, high school took nine years. It was

very hard on my parents to pay it, but they made the sacrifice. When I finished, they couldn't afford to send me to the university, as a result of the inflation. Before, yes, because we were very well off. Then, one day, my grandfather—my mother's stepfather—called me in and said, "Well, what do you want to do?" I told him I wanted to be a paleontologist. He looked at me and he said, "I don't understand a thing about that. I don't even know what you're talking about. But I'll see that you keep yourself clean." [Laughter] He said, "I'll tell you something. I will finance your education until you get your whatever it is"—I said the doctor's degree—and he said, "The day you get your doctor's degree, you're on your own." I said, "That's fair enough." I was very excited about it.

ASPATURIAN: When did you decide you wanted to study paleontology and make it a career?

LOWENSTAM: In high school.

ASPATURIAN: What led you to make that decision?

LOWENSTAM: I spent most of my spare time, and even some of the time I should have used for schoolwork, collecting and preparing fossils. They were marine fossils, and I could see what it was like when you were near the ocean in that area. Clean water with all the teeming life, and the inferno of the environment just faded into the background. That came very early, maybe the third or fourth year of high school. I never realistically thought about a profession. I knew I was going to do something with fossils but had no idea how I could do it; I thought maybe I could do it as a hobby. But there was no question that that was my main interest.

Also when I was a high school student, Alfred Wegener—the man who first proposed the continental drift theory—came and gave a talk in our town. I went to hear him, and that was one of the reasons I got into geology, not only paleontology. He showed how the coasts of Africa and South America fit together, and I got very excited. Continental drift wasn't accepted for more than another thirty years; in America, especially, they resisted it for a long time. But for me, after that, there was no question.

ASPATURIAN: Were you a good student in high school?

LOWENSTAM: No. [Laughter] I was good in biology and geography. Math was fair. Of course, I couldn't see any sense in my studies at the time. Later I did. I just squeaked through.

ASPATURIAN: Did you also read books that strengthened your interest in fossils and paleontology, or was it mostly being out in the field and collecting?

LOWENSTAM: There were also books. The district geologist of the Prussian Geological Survey had heard that I was collecting fossils. He came to the house and looked over my collection, and he saw a number of things he didn't have in preparation for his monograph, which he was working on for twenty years. He asked me if he could have those, and I gave them to him. From then on, he came every year and looked through my collection, and he sent me publications.

ASPATURIAN: He took a personal interest in your collection and in you?

LOWENSTAM: Yes. As a matter of fact, in the early Nazi time, he was very opposed to the Nazis. He belonged to the old Nationalist Party, which was, I had been told, very anti-Semitic. But when Hitler came to power, he changed completely. He did two things I particularly remember: In the early Nazi time, he hired me for a summer as a field assistant, which was totally against the rules with the Nazis. He didn't tell anybody I was Jewish. We went, and he taught me field methods, geologic mapping. The other thing was, when he published his publication I was telling you about, which was during the war, he somehow succeeded in sending me a copy—via Switzerland or so. And in that publication, he mentioned that I had supplied him with very rare fossils and in which area I had found them.

ASPATURIAN: That was a courageous thing to do.

LOWENSTAM: Yes.

ASPATURIAN: During the Weimar period, do you remember anything about the political attitudes of the people you associated with?

LOWENSTAM: They liked what was going on, but they were not actively engaged in it or involved in anything. We were in a very peculiar situation in Upper Silesia. It was the second largest mining district of Germany. Its coal reserves and its lead and zinc ore made this area strategically extremely important. The coal reserves were the largest known in Europe at that time. So at the end of World War I, in order to prevent Germany from building up for another war, the Allies were anxious to take Upper Silesia and give it to the Poles. The region had a small population of Polish people who came as miners from Poland, and who mainly spoke a dialect that was pidgin—half German, half Polish. The Allies tried to use that population as a pretext to take all of the Upper Silesian mining region and cede it to the newly formed state of Poland. The Germans tried to resist, and as a result, after the war ended, throughout the early 1920s, there was an unofficial war going on. The Germans had an army, which was not officially recognized, and so did the Poles. There was fighting going on. I remember one day we were on the way to my grandparents' house for my grandmother's birthday. Suddenly the Poles moved a militia into the end of the street where we were, and started shooting. We crawled on our bellies the next two blocks. People got shot. There was a seesaw battle.

After the fighting between the unofficial German and Polish armies, it came to a plebiscite. The French were anxious to see if by plebiscite they couldn't cede Upper Silesia to Poland. It turned out that the majority was pro-German, but there were certain areas where there was an equal number of votes for Poland and for Germany. So they partitioned Upper Silesia. As a result of that, Siemianowitz, where I was born, was given to the Polish side. And after that, my parents moved to Beuthen, which was on the German side but surrounded on three sides by Poland. We were a peninsula; they even cut mines underground and put walls in, and either the Germans or the Poles opened new mine shafts, depending on where the main shaft was located. We were right on the Polish border.

ASPATURIAN: Do you recall what the feelings were, that parts of your community were suddenly part of Poland? Were people bothered by this?

LOWENSTAM: Oh, yes. They thought it was an injustice.

ASPATURIAN: Aside from the unofficial militias, how were relationships between the Germans and the Poles, living in such close proximity?

LOWENSTAM: They coexisted, you might say. The Germans looked down on the Poles, and the Poles in a way tried to look down on the Germans. They would point out that in the ninth and tenth centuries the entire region had belonged to Poland. And that the Germans, as they moved eastward, had taken over East Prussia, which also used to be a Slavic area.

ASPATURIAN: Were there major class differences, or economic differences that you recall, between the German and the Polish nationals?

LOWENSTAM: Yes. Usually the Polish people in our area were laborers and the Germans were the employers.

ASPATURIAN: What was the feeling of the people around you about Germany's defeat in the war?

LOWENSTAM: We felt bad about it, of course. But I don't remember anyone who was really politically oriented in any way. We were more concerned about the area that we lived in, because my family had lived in this one township for 500 years. So my father knew every place; he took me out to the cemetery, where there were long rows of ancestors. That was the interest, not what had happened in recent history.

ASPATURIAN: Did you have a lot of relatives in the same community, because your roots went back so far?

LOWENSTAM: Yes—in that community and nearby.

ASPATURIAN: Was this a largely Jewish community or was there a Jewish and a Gentile component?

LOWENSTAM: It was basically mostly Jewish. But my step-grandfather became Jewish. He converted to marry my grandmother, which was unusual in those days. That was like my mother being a newspaper art critic. We were a rather crazy family. [Laughter] Almost all his friends were non-Jewish. So I personally saw quite a few of them. My uncle, too—his older son. He had two children, a boy and a girl. Then in high school I belonged to a small circle of students who were quite interested in the political situation, because things started to polarize between the Nazis on the one hand, and the Social Democrats on the other hand. This was in the late 1920s. About nine or ten of us had a little private club. I think I was the only Jew in it.

ASPATURIAN: I assume this was an all-male school?

LOWENSTAM: Yes. [Laughter] We held our meetings in the attic of my grandfather's house, where he gave me a room. If my grandfather had known what we were doing, he would have had a heart attack, because he was totally apolitical. [Laughter]

ASPATURIAN: What kinds of discussions did you have?

LOWENSTAM: We were looking at what would be the best for people in the future. We invited the Communists to talk to us; we invited the Nazis to come to us. [Laughter] We were very objective in those days—it was high school. I remember the Communists came very freely and talked. We had two Nazis come to talk with us, but the others that we invited didn't come.

ASPATURIAN: Do you remember what your reactions were when you listened to these speakers from various groups?

LOWENSTAM: I think I was rather taken aback by the Nazis. I didn't believe in the Communists, and the Social Democrats were pretty far to the left. But what was interesting was when the Nazis came to power, all the Communists among the students and the adults I knew suddenly became Nazis. That shocked me so much that later, whenever any of those people came there, or to any other place, to try to talk to me, I wouldn't talk to them.

ASPATURIAN: What made you join this group in the first place?

LOWENSTAM: There was one student, Hans Kriegish, who was really a very dynamic person. He started to talk to us and say, "We have to do something about the situation. We can't let it go as it is." He got us all organized into this little group.

ASPATURIAN: In the early years of the Nazi Party, were the Nazis and Hitler taken at all seriously among the people you knew?

LOWENSTAM: Some did, but many didn't. I remember that my father was a close friend of the parish priest, and the two would talk about it. I remember the priest saying one day to my father that if anybody ever tried to fight the Catholic Church and the Jews, there was no chance.

ASPATURIAN: Did your father agree with this?

LOWENSTAM: Oh, yes. The priest was usually more politically involved, so my father respected his opinion.

ASPATURIAN: Do you remember when you and people close to you first began to think it was a more serious situation?

LOWENSTAM: It was after the 1930 election, when the Nazis got a very large number of seats in the parliament. We were really very concerned about it, because there started to be street wars, and our group was involved in beating up Nazis of our age. So we took it very seriously. The thing was that at the next election the Nazis lost a great deal. If President Hindenburg hadn't taken the advice of others and asked Hitler to become chancellor, it would have been all over for them. They were really past the peak.

ASPATURIAN: What was the reaction among you and your friends when Hindenburg did invite Hitler into the government?

LOWENSTAM: We thought that was the end. But we didn't realize how serious it was. You see, I was a student in Munich at that time. I was so mad that my future wife was afraid to walk with me in the street because I was ready to beat up those characters just as I had in the town where I grew up.

ASPATURIAN: Can you tell me a little more about the period when your group and the Nazi youth groups collided?

LOWENSTAM: We made it a game in the evenings, when it started to get dark, to assemble and lie in wait for them when they walked in their uniforms.

ASPATURIAN: Was this group of yours made up of people in your high school?

LOWENSTAM: Yes. Although one member of the group—my closest friend, who was also Jewish—was from the *Gymnasium*, not the *Hochschule*. He was six-foot-four and built like a tank.

ASPATURIAN: How about the Nazi groups? Did they also come out of the local high schools?

LOWENSTAM: No. Mostly from the workers.

ASPATURIAN: So these were not people whom you had known earlier as you were growing up, in any capacity?

LOWENSTAM: Only two who were in my school. The rest didn't even speak German; they spoke pidgin German and wore Nazi uniforms. I've never forgotten what happened to my grandfather after they got into power. He had two big stores, because he did not only decorate castles. There were local well-to-do upper class people who had things like sofas made there and bought carpets, and he did special curtains for them. And suddenly, there were two Nazis in uniforms standing in front of his store and speaking pidgin German. That was the irony.

ASPATURIAN: Originally, in the late twenties, these people were not taken very seriously?

LOWENSTAM: Many in the late twenties were still Communists. Since we had steel mills and mines, we had a substantial working class, and they had a very large Communist contingent. And they would demonstrate. And then they turned around, and became Nazi supporters.

ASPATURIAN: When did the turnaround happen?

LOWENSTAM: As soon as the Nazis came to power.

ASPATURIAN: Did most of the Communists you knew become Nazis?

LOWENSTAM: I knew of two. They rode by one day on bicycles in the Nazi uniform, claiming that I shouldn't take that too seriously. They were just lying low for the time being. One I never trusted. The other one, I think, got killed by the Nazis.

ASPATURIAN: So all this started happening while you were in high school.

LOWENSTAM: It started, yes. But when Hitler came to power, I was at the University of Munich. I had no idea he had come to power. I was skiing in the mountains. A bunch of us had been living in ski huts for six weeks, and the ski lodge was isolated. I came back down from the mountain in the spring of '33, and I felt the excitement, and I said, "What's going on?" And people said, "Don't you know? This great revolution occurred. Hitler has become chancellor." I wasn't there during the takeover. I had no idea.

Begin Tape 1, Side 2

ASPATURIAN: You started at the University of Frankfurt?

LOWENSTAM: Yes. For a while, the fossils I collected were all fossil reptiles about 200 million years old, so I got interested in vertebrate paleontology. There was a famous man in Frankfurt, Professor [Fritz] Drevermann, and I wanted to study with him. I arrived right on the day he was

buried.

ASPATURIAN: You did not know he died.

LOWENSTAM: No. It was rather sudden. It was a great shock to me. Then I started to take courses in geology in general, and invertebrate paleontology; and I had one unofficial course in vertebrate paleontology because there was Tilly Edinger. She was only a lecturer. Her father was a very famous neurologist. Her interest was to study vertebrate fossil brains by making casts of the brain cavities. That was rather fascinating for a while. But then there was infighting over who would be the successor to Drevermann—who would also take over the Senckenberg Museum. And a politically oriented fellow was chosen instead of the man who should have taken over, Professor [Rudolf] Richter. So [Richter] quit teaching at the university. He continued teaching at the museum only after we begged him on our knees not to abandon us, and then at the end of the term he left. He told us to leave Frankfurt.

ASPATURIAN: How many of you were his students?

LOWENSTAM: Half a dozen, and we all left. Two of us went to Munich.

ASPATURIAN: You went from Upper Silesia to Frankfurt to Munich in one year; quite a bit of moving around. Had you traveled much before that?

LOWENSTAM: I did; my parents didn't. I was in a Jewish youth organization that did a lot of hiking and walking and sleeping out. So I got around quite a bit.

ASPATURIAN: Where did you travel with this group?

LOWENSTAM: Pretty far away from Silesia; mostly western and southern Germany—Bavaria. When I was in high school I had a good friend, and the two of us did things others wouldn't do. He took me once to Berlin because his uncle—I don't remember if he was Berthold Brecht or Kurt Weill—was there. We peeked in on the stage while they were preparing the *Threepenny*

Opera, and I saw Marlene Dietrich. We were there about two weeks. They treated us very nicely, even though we were high school students.

When I was a university student, I started to travel to places like Italy. In 1934 Mussolini had an international art exhibit in Venice—he tried early to show his cultural side. If you got the ticket in Munich for about the equivalent of $20, it covered the round trip and allowed you to interrupt the trip anyplace along the way. For me, that was just great, because Alexander von Humboldt had described a region in northern Italy where you could see how volcanic material that came up had affected the host rock into which it was injected. I saw that on my way to Venice. When I interrupted my trip there, I ran into a private mineral collector who had some very rare pieces with beautiful minerals. He took me along somehow; I've always been surprised that he did. Then on the way south, before I got to Venice, I took a side trip to Recoaro in northern Italy, where I collected fossil sea lilies, from the same period as the ones I collected where I grew up. The ones in the area where I grew up were supposed to be related to those from northern Italy. Nobody had been there for about fifty years. So when I came out into the middle of nowhere, it was just wonderful, because all of the fossils were weathered out and nobody had ever collected them. I got a fantastic collection.

ASPATURIAN: Would you say that one of your major memories of your first trip to Italy was of the geology and the fossils?

LOWENSTAM: No, the art, too. I was talking yesterday about it. I was wishing I could find out—and somebody told me how I could yesterday—where to find a catalog of that exhibit, because there was one section essentially on the evolution of perspective in early Italian paintings. It was just fantastic. It showed various experiments; some were rather crude. I was fascinated, following it all. I spent more time on that part of the trip than on any other. Later I went to Florence.

ASPATURIAN: Was this your first trip outside Germany?

LOWENSTAM: No, I had been previously once in Poland, in Cracow.

ASPATURIAN: What had taken you to Poland?

LOWENSTAM: The trip gave me a chance to see an underground cathedral made out of salt crystals in an abandoned salt mine. I went by train with an excursion group.

ASPATURIAN: How old were you?

LOWENSTAM: Maybe eighteen.

ASPATURIAN: How did you find out about this?

LOWENSTAM: From a Polish border guard who knew me because I collected fossils across the border.

ASPATURIAN: You could do that very easily?

LOWENSTAM: Yes, until Hitler and Poland signed a friendship treaty. From that day on, I had real trouble. I had a Polish policeman on bicycle following my bicycle wherever I went, and he had to make a report on what I was doing over there. But up to that time, I was able to go over there. I knew certain border people who knew what I was doing; I showed them the fossils. They were curious, but the guard said I could explain it to them.

And one day he told me about the salt mine and said, "You ought to see it. It's just beautiful." That part of Poland used to be part of Austria. So was Silesia, by the way. Silesia was taken away by Frederick the Great in the Seven Years' War, where he almost lost his pants and everything else, fighting Maria Theresa. Maria Theresa had had this abandoned mine converted into a kind of cathedral with huge candelabras. Instead of having quartz crystals, the candelabras were made of salt. Everything was made of salt. People came from all over Europe to see this. It was really intriguing. And I often used to go skiing into the Tatra Mountains, which were in southern Poland. My uncle had given me a pair of skis, so I taught myself skiing. It was very helpful when I became a student in Munich, because field work had to be done in the high Alps. We "flatland Indians" were training doubly to outdo the local people, who were born

on skis. [Laughter] I'll never forget one field trip I went on with a friend of mine. We were both from the flatlands. We dragged a couple of fearless Alpine boys all the way up to an elevation where they couldn't take the altitude and became unconscious. We wound up right below the Matterhorn, in Switzerland it was, about 1,000 feet below the peak. We were very proud.

HEINZ A. LOWENSTAM

SESSION 2

June 28, 1988

Begin Tape 2, Side 1

ASPATURIAN: You enrolled in the University of Munich in what year?

LOWENSTAM: I came in ’33.

ASPATURIAN: Were you familiar with that area of the country?

LOWENSTAM: No, but I knew Munich was famous for its school of paleontology. In those days it was the leading one in Germany—an international school where people from England and the U.S., along with the Germans and the French, studied. So I went there. My professor there was a rather weird man—Professor [Ferdinand] Broili. But my real mentor was Edgar Dacqué. He was of old Huguenot ancestry and still spelled his name with an “e” that had an acute accent. He was a very curious person—an interesting mixture of German and French. He had the *esprit* of the French and the build of a huge German. He wrote the first book on comparative functional morphology of invertebrates, instead of simply describing fossils. He was interested in the biology and what one could learn about the organs. And those were my interests, really. He was the chief curator at the university museum, but he wasn’t allowed to teach. Broili’s predecessor was Professor [Karl A.] von Zittel, a brilliant paleontologist and a very famous man in his day, and when he died Dacqué would have been the logical person to take over. But Broili, who was trying to get the position, knew how to annoy Dacqué, and Dacqué got very excited and said, “Take it, I don’t want it,” and went to the museum and retreated from teaching. That’s what Broili had wanted.

Dacqué was my real mentor. Every so often, he would come into the geology library and say, “Stay away for a week or so, because Broili is on the warpath; he says you are his student, how dare I have you here?” [Laughter] So I would stay away for a little while, but he was really my major influence.

Broili was a very dry, taxonomically oriented man. He, too, was very curious in his

behavior. I never knew the source of it until I was assigned to natural resources development by the U.S. government during World War II. I was stationed in Urbana, Illinois, and I met the [Maurice] Goldhabers there—a couple who were both physicists. Mrs. Goldhaber was from Munich, and she turned out to have been the girlfriend of Broili's daughter. I mentioned once how weird he was, and she told me that his brother had become insane and committed suicide. Broili was afraid the condition was hereditary, and so he was watching himself. That started to make sense about a lot of things.

ASPATURIAN: I did want to ask you to talk about the social and intellectual climate you found in Munich in the early 1930s.

LOWENSTAM: First of all, I lived in Schwabing, which was the art colony of Munich. I lived in the apartment of an American bachelor who published illuminated manuscripts from the Middle Ages that hadn't been previously published. Like me, he was the black sheep in his family; that's why they sent him to Germany from New York. Through him I met quite a few interesting people. One of them was Erika Mann, the daughter of Thomas Mann. I became acquainted with quite a few of the young artists who came to the cabaret and the whole atmosphere in Munich.

ASPATURIAN: Did you purposely settle in that part of the city because of the atmosphere?

LOWENSTAM: No. I drifted over there—I always drifted into things—and then I liked it. It wasn't a deliberate choice. But after a little while, I wouldn't have wanted to be in any other place. I met some curious people there. One was an old—I think he was Prussian—officer of the nobility, who patronized me greatly. We had lots of philosophical discussions. He was obviously no Nazi or inclined in this direction. And my fellow students at the university were exceedingly nice to me. There was one in particular I'll always remember. We would attend classes together and when we came out of the old academy building, one of [Julius] Streicher's anti-Semitic newspapers would be there in front.

ASPATURIAN: *Der Stürmer*?

LOWENSTAM: Yes, that was it. And I always knew when there was a particularly bad article, because he would get me engaged in a discussion before we got out. As we came down the steps, he would drag me along, discussing something until we were way past the entrance. He was very nice. I was glad to see him again in '81, when I got an honorary doctor's degree at Munich. He came to that, which was very nice. He was probably the least sophisticated of the whole group I was with. But he had his heart in the right place.

ASPATURIAN: When you started in Munich, what was the political situation in Germany?

LOWENSTAM: It was pretty mean. It was on the brink of a major change. But Munich was unique. The people in Munich always made the statement that they saw Hitler in the first putsch, when he said he would rather die at the head of the column than any other way—and he was the first to run when things got bad. They never really respected him after that. I often heard that from old Bavarians, who were mostly royalists. In fact, Professor Broili said Hitler loved Munich but Munich wasn't fond of Hitler. After the Nazis came to power in 1933, one assistant of Broili would have to go in Nazi uniform once a week to something in order to keep his assistantship. The first time I saw him I was really aghast. He called me over and said, "You know me. I live three-quarters of the week in my office and can work, and the last part of the week I go out and eventually get drunk, and the policemen know me and pick me out of the gutter and bring me back. I'm not politically oriented." He would tell me anti-Hitler jokes—like, he came in one day and said, "I heard a good one today. Everybody stands behind Hitler, but nobody would want to stand in front of him." [Laughter] So the whole atmosphere in Munich—in the circles where I was, at least—was quite different from Berlin, for instance, or Prussia, in general.

I told you I was more or less an *enfant terrible*; I would do things other students wouldn't dare do. I got interested in certain fossils and I wanted to see the living representatives. So I went up to the office of the professor who was in charge of recent marine organisms and knocked on the door. He asked me what I wanted, and I told him.

ASPATURIAN: Was this something that would normally not be done?

LOWENSTAM: Right. No student would; students were dirt. But he was delighted, and he showed me the collection, and he gave me all kinds of things to take back for comparison with what I was doing. I also remember taking a class from [Erich] von Drygalski, who taught a course in geography. Nowadays, no geologist would deal with geography. I found it very interesting. He talked about Asia, and China in particular. He started with the geology, went on to how the regional geology and morphology influenced the cultural development and divisions in China, and then went into the cultural history and a little about the relationship with the European nations and so on. I was fascinated by this. I don't remember how I got to know him personally, but when I went to Palestine later to do research for my thesis, he loaned me his altimeter—an instrument to measure elevations—that he had used when he was on the German Antarctic Expedition. There were no topographic maps of Palestine when I went, and he just handed it to me and said, "You will need this, because there are no maps. So if you do your geologic mapping, you better have this." Which was, again, unheard of. I also took a course with [Karl] von Frisch—the man who studied the language of the bees. Not only did I take a course by him, but I'm glad I remember it now. I had come back from skiing in the mountains, after Hitler took over, and I came to the university. Von Frisch saw me. He took me by the hand; he didn't say a word. He took me into a room in his suite of labs and he said, "From today on, you can work here whenever you have anything to do." That's all that he said. The curious thing about the story is, it stuck so much in my mind because I was so impressed with the way he handled the situation, to say, "I'm not a Nazi." But he didn't remember it at all. I told Mark [Masakazu] Konishi here on campus this story. Konishi was a friend and fellow student of one of von Frisch's students. He said that when this student asked von Frisch about it while he was still alive—he was then in his nineties—he didn't remember me; he had no recollection of the incident at all.

ASPATURIAN: What was the course he was teaching that you took?

LOWENSTAM: I think invertebrate animal behavior. I took all kinds of courses.

One time I got interested in the comparative anatomy of the vertebrate brain. I started with sharks, dissected some sharks in biology and went up the ladder, and then I came to man. So von Frisch told me to go to the anatomy medical school. I went over there and saw von

Wassermann. He said, "Yes, young man, I have never seen you around here. What can I do for you?" I said, "I'm interested in the comparative anatomy of the vertebrate brain, and I would like to take a look at the human brain." He looked at me; he said, "This is very irregular. You're not a medical student, are you?" I said, "No, I'm in geology, paleontology." He smiled and said, "All right, when do you want to start?" Just like that. That's what I found so intriguing. Students, as a rule, wouldn't have the guts to even think of going. I didn't try outlandish things—I thought I had a legitimate reason whenever I went to experts outside my field. These people were actually very accessible.

ASPATURIAN: You had said earlier that students were treated like dirt. In what way?

LOWENSTAM: They were nobodies in the system. I'll give you an example. When Broili had field trips, he was always well dressed, a collar and tie on, with this Bavarian hat with a feather on it. His assistant, Neugebauer, was dressed the same way—with knickerbockers, you know. When we came into a town, Broili and Neugebauer would automatically go on the other side of the street so that they wouldn't be associated with the gang of students, who were not properly dressed according to their standards. That was the general pattern.

Many students studying at Munich weren't very interested in the university. There were these fraternities. There's a famous story of one fraternity where there were four students who had signed up for von Frisch's course but never went; they selected one to go to class. So one of the other three students, who had only the notes, didn't know that von Frisch was a specialist in the language of the bees. He was a pre-med student, and one day von Frisch asked him to come for his exam. He said, "You know, I don't have time to come in and give it. You come out to my house." The student went out there, and there was a fenced-in porch, and he opened it, and there were bees flying around. And he started to slap them away. Well, he had flunked the exam from the very start. [Laughter] But it reflects some of the general attitude, which started with the professors. They didn't have much interest in the students. Part of it was that students had to pay per course, and part of the professor's salary depended on what was paid by the students. Many of them had other interests, they really didn't want to teach. But the geologists were very interesting.

Same way with mineralogy. I didn't have to take mineralogy, but I took a detailed course.

There was no professor, only an assistant. He was very conscientious and he asked me, "Why are you taking it?" And I said, "I just like to. I don't know why. I'd like to learn something about it." When I got through, I said, "I would like to have a slip that I took the course." He looked at me and said, "If you want one, I'm glad to give you one." But, again, people were pretty specialized. None of the paleontologists knew mineralogy.

ASPATURIAN: Your PhD was ultimately in paleontology?

LOWENSTAM: Yes and no. You see, I was going to write my thesis on something in paleontology. I wanted to work in paleoecology, but that was unheard of. Dacqué understood it, but Dr. Broili, my official teacher, found out that I had gone to the Prussian Geological Survey in Berlin and had gotten an idea for a study on some fossil crinoids—sea lilies.

ASPATURIAN: What year are we talking about?

LOWENSTAM: '34, '35. There again, it was interesting. The people at the Prussian Geological Survey and Museum were awfully nice to me. [Otto H.] Schindewolf, a very famous paleontologist who was later the head of the paleontology department in Tübingen and later became the director of the university, was the paleontologist at the Survey. I was looking for a certain type of crinoid that had been described in 1830 or '40. He had no idea where it was; he wasn't interested in crinoids. But he went around and finally found it. It was covered with dust, but he located it and brought it to me so that I could look at it while I was there. I wanted a big slab to study in detail and photograph, but a student couldn't do that. So one of the professors went to the director and had it sent to Dacqué, so that I could use it in my study.

At that time, since they were so nice at the Survey, I went to Professor [Hans] Stille, the head of the geology department at the University of Berlin, and asked him, just theoretically, what would happen if I applied for a transfer from Munich to Berlin, because of the collection. He went to his double door; he closed it. He said, "Don't do it. I'm under suspicion already by the Nazis. If you come here, that will be my end."

ASPATURIAN: So you went back to Munich.

LOWENSTAM: Yes.

ASPATURIAN: Going back to 1932 and ’33, do you recall much political agitation by the Communists or the Nazis?

LOWENSTAM: There were lots of street incidents—fighting in the streets. Shop windows were broken—some deliberately and some by accident, when the policemen came.

Munich was very Catholic, and also truly Catholic, because a large segment was against Hitler, as long as they could be, because of their religion. And there was Cardinal [Michael von] Faulhaber, who had his church three doors down from where I went to the old Academy. A year or so after Hitler came to power, he gave three sermons on the anti-Christ in such a way that there was no question who he was talking about. The church was practically choked with people trying to listen to him. People were standing out in the street. Of course, Goebbels tried to get him. But the Pope, at the last minute, decided to make him a papal nuncio, which meant he had diplomatic status, so they couldn’t touch him. So it was very fortunate that I stayed where I was.

I should say that very early I tried to get out. I went to Florence in late ’33 and talked to Professor [Michele] Gortani. He was a very nice man. He was not all there. But our interests were similar; he was also a paleontologist. But he said, “No, that wouldn’t work, going to Florence.” Then I went to Strasbourg and I tried to apply to the university there. I had a friend from my hometown who studied there. And I was told that a new edict had come from the French government that all the foreigners, the refugees, had to go into the interior of France. They didn’t want any in the border provinces, such as Alsace-Lorraine. Whoever I talked with said, “Why don’t you go to Nancy?” So I did, and I met a Professor Levi, who was Jewish, which was unusual in France. There were not many Jews in geology; I think he was one of two. In Germany there weren’t any. Oppenheimer was privately wealthy, he had his private collection, and published in German publications. There was another one, whose name I forget, who was baptized in order to become a professor.

So I went to Nancy, and everything was very nice. The professor invited me to his home for the afternoon to have some wine with him. When we were alone, he said, “I’ll give it to you straight: You can come here; you can study here; you will get a doctor’s degree. But in French

terms, this doctor's degree is not the same as for a Frenchman. When you graduate, yes, you will get a job. Where? Maybe the Cameroons, or any part of the French Empire where there's a lot of malaria and other troubles and where the French geologists don't want to go. You will get a job. I just want you to know what it was all about."

So I went back to Munich, and I stayed.

ASPATURIAN: What happened to this man and his family?

LOWENSTAM: I never found out.

ASPATURIAN: In 1933, when Hitler came to power, what was the reaction in the university? How did it affect the academicians and the students? Were there Nazis among those people?

LOWENSTAM: There was [Erich] Kaiser, who was the head of the geology department—a wonderful man and great scientist, who was way ahead of his time. What he published in the late twenties was only recognized later as pioneering science, but in those days nobody paid any attention to him. He mapped the diamond desert of Southwest Africa, which was a German colony. There was no vegetation, no water, and he wrote an article saying that here one could see the processes that had existed on Earth at the beginning when it was formed. He went into detail. And today nobody quotes him. It's very interesting.

But Kaiser died—fortunately before the Nazis came to power, because he made some very nasty remarks about them. He was a German nationalist but not a supporter of Hitler. His successor at Munich was a man who was an unknown little professor in an Austrian agricultural school. If I remember correctly, he was in hiding during the Schuschnigg government in Vienna, because he was on the list of wanted subversives, and was brought into Munich by the Nazi underground. His name was [Leopold] Koelbl, and he immediately became the head of the department in Munich, immediately president of the Bavarian Academy of Science, and all kinds of other things.

ASPATURIAN: His support for the party catapulted him into these positions?

LOWENSTAM: Yes. He was aghast to find that a Jew was in the Munich geology department. First he tried to isolate me from the students, and it didn't work. The students stuck with me.

ASPATURIAN: How did he try to isolate you?

LOWENSTAM: I don't know what he did in detail. All I remember was, he tried to, I think, get them riled up against me, and they didn't play ball. There was only one student who was what you might call chicken—he didn't want to go one way or the other. He wasn't unfriendly to me, but he would talk as little as possible. He was the only exception.

There was a Catholic monk who was a student at that time. On field trips, he would always give me a wink to come forward and then take a bottle out from under his cassock. He was a Benedictine, so he carried the real Benedictine drinks. He would say, "Have a little bit. I wouldn't give those Nazis anything—that professor and his assistant would choke if I gave them any." Then he would fall into line again.

ASPATURIAN: The professor had an assistant who shared his political sentiments?

LOWENSTAM: He brought him along from Vienna. So when he couldn't isolate me among the students, he turned around and acted friendly. To give you an idea of his behavior, when I left Germany, we had an encounter. You see, just before I left for America, in 1937, they had accepted my PhD thesis and set the date for the exam. And the week before the exam was scheduled, a new law came out that Jews couldn't get the doctor's degree. So Dacqué wrote a letter on official university stationery, with the Nazi university seal on it, saying I had fulfilled the qualifications of the PhD but due to political circumstances they couldn't give me the diploma. He went over with me to Broili, who was the head of the paleontology department, to have Broili sign where he had typed out his name. Broili sat down and signed. Within ten minutes, Koelbl asked me to see him. I came in, and he said, "I would like to see the letter that you just got from Broili and Dacqué." I said, "What letter?" He said, "Don't be silly." He went over to my pockets and he knew in which pocket I had it. He pulled it out, read it; his eyes popped out, he got mad. He gave it back to me and said, "Nice letter, isn't it." He knew I was going to leave within a week or two. I said, "Yes." He said, "I want to give you a letter of

recommendation, too. But you must not show it to the *Chicago Tribune*, because you know what would happen to me if you did that." I said, "I don't want your recommendation." He didn't listen. He sat down and wrote the letter—I was a good student, in general terms—and gave it to me. As I went out the door, he said to me, "You cannot go to America and say that we mistreated you, can you?" So apparently he had changed in his own mind, after he couldn't force something without a major showdown. He had decided it was the better, smarter policy to have a Jew go out and say, "The stories about the Nazis—the Nazis aren't as bad as all that." He was a clever character, you know.

ASPATURIAN: Were other departments and official positions in the university affected in the same way, in terms of Nazi sympathizers being brought in?

LOWENSTAM: I don't know. He and his assistant were the only ones that I was aware of. I'm sure there were others.

I had an interesting experience with an American student from Yale who came to Munich. He had been sent by one of the industrial labs here to study lead, something like that. We became friends in no time, and then suddenly I hardly ever saw him. Finally, he would ignore me completely. I was rather disturbed about what was going on. Then, shortly before he was about to go home, he told me what had happened. He had been patronized by the Nazis in an unobtrusive way. People who had no uniform and never talked about politics officially would invite him to their homes, and gradually try to get him to adopt the general Nazi-type of outlook, without propaganda. He said he finally caught on to what was going on. That's when he came back and talked with me again. But they had permeated him that much without his being aware of it.

There was an interesting parallel in Edgar Dacqué. At one time, he came and threw a little paper on my desk. I looked at it, and basically what it said was, "Germans have one way to look at geology; Frenchmen, another way; Englishmen, a different way." I was so annoyed about it that I went right to him and I said, "I guess you've swallowed the Nazi ideas, haven't you?" He looked very disturbed at me and said, "What do you mean?" I said, "Here. You were just talking like Koelbl over there in the next building—Aryan science." He was really shaken up. He didn't say anything, but a few days later he came in and said, "You know, it's amazing how

things creep up on you without you knowing."

ASPATURIAN: Did you ever feel personally threatened during this period by what was going on?

LOWENSTAM: Once. When I was still at home, I used to go at night into the parks. I liked to see nature at night and what was going on. A gang of Nazi sympathizers ran into me, and they were going to beat me up. And somehow—I still don't know how I did it—I talked them out of it. [Laughter] I still wonder what I said. I remember a few things: "Now, look, you, over there. You live on the next street from where I live, don't you? We knew each other for a long time. What did I do to you, or what do you have against me?" And so, gradually, I somehow diffused the atmosphere, and the gang leader said, "OK, let him go." I never thought I would be able to do a thing like that.

ASPATURIAN: I imagine you went home to your family for visits. After '33, what was going on there?

LOWENSTAM: It was very bad. It was bad because the Nazis were not entitled to do anything in Upper Silesia. After the plebiscite I told you about, when part of Upper Silesia was ceded to Poland, there was a Geneva agreement put in by our chief rabbi, Koptstein—a clause that the Jews on both sides of the border would be treated equally. The Catholics on the German side wanted to be legally protected, and the German Protestants on the Polish side wanted the same thing, and so he had tacked the Jews on as well. So legally, the Nazis couldn't do anything, but they did all kinds of things. They boycotted my grandfather and father's place.

Begin Tape 2, Side 2

LOWENSTAM: I think my father, together with the Catholic priest who was his good friend, thought in time that things would get better. My step-grandfather, who had become Jewish, was totally apolitical—he wouldn't have known what the new world meant. The Nazis came to him and said that if he divorced my grandmother he would be reinstated as an Aryan and could help them build and decorate the big government places. My grandfather was so disturbed by it that during that week he and my grandmother committed suicide together.

ASPATURIAN: When did this happen?

LOWENSTAM: In ’38. I had left by then.

ASPATURIAN: And your parents, had they left?

LOWENSTAM: No. My parents left when the war broke out.

Well, there was an interesting thing about my PhD. During one field trip with Koelbl, there was a big discussion among the students, and he was pounding the table and saying, “German things must be done by Germans.” A few minutes later, without even remembering what he had said, he said to me, “Don’t you think it’s about time to think about the subject for your doctor’s dissertation?” I was just boiling, and I said, “Yes, I want to go to Palestine and write a doctor’s thesis on the subject of Palestine.” He was ready to pounce on me.

ASPATURIAN: When was this, now?

LOWENSTAM: ’35. He was ready to jump at me. And then he smiled and said, “OK.” I remember coming home that night to the apartment in Schwabing, and my friend the landlord said, “You look awful. What happened today? What did you do today?” You see, before, I always had my mouth closed, but lately I had shot my mouth off at the university. So he would question me every evening about what I had said that day. Half the time we would move at night; he was sure the Gestapo would come and pick me up. I could never keep my mouth shut. So when he said, “What did you say today?” I told him, “I shot my mouth off, but I can’t produce. I don’t have the money to go to Palestine.” He said, “Don’t worry. I have friends in New York. They will take care of it.”

So I went to Palestine for a year and a half. Earlier, when Edgar Dacqué and I had talked about possible thesis topics, he said, “You want to do your dissertation on a paleontologic subject, don’t you?” I said yes. He said, “No. You take an area and map the geology of it, including some volcanic and igneous rocks, if they are there. Then, when you have done all of that and integrated the geology and structure and history of the territory, then you do your

paleontology." I don't know why I listened to him, but it was the best thing that ever happened to me. I learned the significance of field information, which today is widely abandoned in favor of the computer, which runs the same old data in new forms instead of giving new information.

ASPATURIAN: On a totally different note, was the University of Munich coeducational?

LOWENSTAM: Yes.

ASPATURIAN: Because in our last interview, you mentioned your future wife. I wondered if you had met her there.

LOWENSTAM: My first wife, yes. She was in medical school. She got her doctor's degree the week before the edict against Jews getting advanced degrees came out, which was fortunate, because in medicine, she would have had to start over from scratch. I should say that before I went to Palestine, we discussed whether we might want to emigrate there. I remember while I was gone, she was taking courses in Hebrew. She had an uncle in Chicago who was going to give us an affidavit, so we knew we could also go there. But we were thinking of going to Palestine originally.

ASPATURIAN: How did you meet?

LOWENSTAM: Just accidentally in one of those student restaurants—a vegetarian restaurant. I wasn't really a vegetarian, but I was told this was the restaurant where vegetarian meals tasted better than any meat dishes and were cheaper. [Laughter] That was very important in those days, because I saved money from food and other things so that during vacation I could bum around and do interesting things.

ASPATURIAN: Did you know anyone who left as early as '33 or '34, not intending to come back?

LOWENSTAM: Yes. One friend of mine went to Strasbourg. Others had started to make preparations for leaving even before the Nazis came to power. I lived in one house where the

son of the lady had a grand piano, and he was going to Palestine. Before the Nazis came to power, he started buying gold coins—often antique gold coins, which were more expensive then. He converted everything into gold coins and he hid them somewhere in the structure of that grand piano. And I remember when I came later to Palestine, the piano arrived safely with all its coins.

ASPATURIAN: He took a big risk.

LOWENSTAM: He did. Because when we left, the Gestapo came and went through all our books. And I think they were really looking for money hidden in those books.

HEINZ A. LOWENSTAM

SESSION 3

July 6, 1988

Begin Tape 3, Side 1

ASPATURIAN: What happened once you decided to go to Palestine for your doctoral research? How long did it take you to make arrangements to go overseas?

LOWENSTAM: You know, I have no recollection. It was apparently fairly simple. I went on an Italian ship from Trieste to Haifa. The food was terrible. The rumor was that the ship might turn around any time and go back, because the Italians had taken over Ethiopia, and there was the possibility that the British would react and there might be war. So there was a rather curious atmosphere.

ASPATURIAN: Did you have any trouble getting out of Germany because of where you were going?

LOWENSTAM: No. It was straightforward.

ASPATURIAN: Was it unusual in those days for a geology PhD candidate to go out of the country to do the dissertation?

LOWENSTAM: As far as I know, it was unusual.

ASPATURIAN: How did your advisers react to it?

LOWENSTAM: Oh, they thought it was all right. As a matter of fact, Edgar Dacqué had done research on the fossils of the same period I studied, mostly in Libya and the Libyan desert of Egypt, and published on it. So he was quite intrigued by the idea I would be going to the Mideast.

ASPATURIAN: Did you have an arrangement with Hebrew University in Jerusalem, or were you entirely independent?

LOWENSTAM: I was in contact with, and advised to a certain extent by, Professor [Judah Leo] Picard, who was the only geologist and paleontologist at the Hebrew University at that time. He even thought I would be coming back. He was a rather devious character, I found out in time. His assistant, who later became the head of the Hebrew University geology department and director of the Geologic Survey, told me never to let Picard have my maps because they would disappear or he would do something with them. [Laughter]

ASPATURIAN: What was the topic of your thesis?

LOWENSTAM: The geology of the eastern Nazareth Mountains of Palestine. That is the southern border of Galilee, quite a bit below the Golan Heights. The eastern part, Mount Tabor, is a semicircular mountain; it has a big monastery.

ASPATURIAN: What was particularly geologically interesting about this region that you wanted to study?

LOWENSTAM: Actually, Picard had mapped the western Nazareth mountains. I thought it would be interesting to see the continuation toward the Dead Sea Rift Valley—the relation to the Rift Valley. That was the pivotal area, where the Emek, which was on the valley border, came up against a big rift that came from East Africa. It was very interesting to date some of the movements that took place.

ASPATURIAN: From a paleontological standpoint, were you looking at the fossils in the area as a means of dating?

LOWENSTAM: Yes. I used the fossils for correlation and age assignment.

ASPATURIAN: There was not carbon-14 dating back then?

LOWENSTAM: No, no. The radiometric age determination didn't exist at that time.

ASPATURIAN: So dating was done just from the strata?

LOWENSTAM: Yes, and the fossil content and how it correlated with other areas in Egypt and in the Balkans. Some fossils would even correlate with Bohemia, in central Europe.

ASPATURIAN: Where were you staying during this period? Where did you live?

LOWENSTAM: Well, I started off in Kibbutz Ginnegar, in the Emek, which was just founded. We lived in tents, in deep mud—it was rainy season. It was miserable. I used that as a base. I did everything by foot, initially, later by horse and donkey. And from there I moved up to Kafr Kana, to the Greek Orthodox monastery. If you paid a little bit, they took you in. The Roman Catholics, across the alley, wouldn't take me, but the Greek Orthodox did. Their main interest was that one of the flock, one of their people, would be my guide. I would pay him for that.

ASPATURIAN: Did you speak Hebrew or Arabic?

LOWENSTAM: No.

ASPATURIAN: Did you learn any while you were there?

LOWENSTAM: I learned Arabic in the end, when I lived among the Bedouins. From Kafr Kana, I moved to a settlement that was founded by Baron Hirsch for old Jews who came from Europe to die in Palestine. It was a very weird place. From there I moved to the Bedouin camp—it was behind Mount Tabor.

ASPATURIAN: What prompted you to move to the Bedouin camp?

LOWENSTAM: Well, I was always advised by the British district commissioner—this was the

period of the British mandate. He was an exceptional person. He was interested in the people he administered, and he spoke Arabic. He was a friend of the Bedouin sheik. And when I told him I was going to map that area, he said, "Well, you have two choices. You either need to have police protection—the Bedouins are very good shots—or, since I'm a friend of the number-one sheik, you can live with them. You are my friend and he is my friend, and you will become his friend." I decided it was much more romantic to live with the Bedouins, and that's where I learned Bedouin Arabic in a hurry. I'm very poor in languages, but I learned very fast, because there was no other way to communicate.

ASPATURIAN: For how long a period did you live with them?

LOWENSTAM: About two months, while I was doing my field work.

ASPATURIAN: What was life with the Bedouins like?

LOWENSTAM: At the beginning, it was very difficult. I almost left, because I was literally covered by fleas and I didn't sleep for four nights. By the time I decided I couldn't take it, they suddenly decreased. I got some kind of an immunity perhaps. No medical people believe it, but I know it from experience; my flea population density went down to this. [Laughter] So I stayed and I learned Bedouin Arabic. It was a very tight society of people living in subgroups. We lived in those tents. When I arrived there, an Arab police officer took me over. All the way, riding to the tents, he tried to talk me out of going. He said, "They are pigs; they are dirty. You can't live with such people; they aren't even people; they're animals." The peasants were all against the Bedouins, because Bedouins would drive their herds any place, and the townspeople, like this officer, looked down on them like aborigines. So we arrived. The sheik came out, and had a rusty can of camel's milk, and he rolled this cigarette and licked it and handed it to me. Apparently I tried to step back involuntarily. The officer stuck his thumb into my back and whispered in my ear, "If you want to get killed, you asked for it. I don't want to; you take it." So there was always this real division. The Bedouins actually lived a very natural life—I thought they were doing very well. They had little huts. Once, at the beginning, I asked if they would sleep in them when it rained, and they thought it was very funny. They would suffocate,

they said. The huts were for the goat cheese, which was their currency.

ASPATURIAN: Where did they sleep?

LOWENSTAM: In camel-hair tents, goat-hair tents. The sheik's mother, for instance, would come and beg me for cigarettes. If he had caught her, he would have killed her. She was not supposed to enter our tent.

In time, I learned Bedouin Arabic, and even wrote some petitions for them to the district commissioner. They asked me to; they could not read or write.

The sheik's oldest son was my guide. When I was out in the field, we always stopped at ruins—at tels—because the British insisted that I was looking for antique stones, I wasn't doing geology.

ASPATURIAN: Why did they insist that?

LOWENSTAM: They were worried, because oil had been found in Palestine in one spot. It later turned out not to be much, but they kept it to themselves. Everybody anticipated that the coming war would affect Palestine in a major way, and they were afraid that if it was now known there was oil in the area, things would be even worse. They wanted nothing to do with it. So usually, while I worked in the mountains, the peasants would bring me amulets and coins and give them to me. I had a fantastic collection of coins, going back to Maccabean times. Sukenik, the first director of the Palestine Museum, which later became the Israeli Museum, was a fantastic man. [E. L. Sukenik, the father of the Israeli general and archeologist Yigal Yadin, was a well-known archeologist. He is best known for being the first to recognize the significance of the Dead Sea Scrolls and for acquiring them for the Hebrew University in 1947.—ed.] Every time I came to Jerusalem, he would go through my collection, and he would say, "Oh, we don't have this coin" or "We don't have this amulet." And I would give them to him.

ASPATURIAN: Are you listed as the donor where these things are exhibited now?

LOWENSTAM: No. But the nice thing about it was that all I brought back to Germany were the

dregs of the collection, so to speak. When the Nazis confiscated what was left of it when I left Germany, I almost laughed, because they only got the things they didn't want in Palestine.

That reminds me of a funny story. One day, when the sheik's son took me out, we crossed the caravan route and a caravan came, and the son of the sheik knew the guide of the caravan. They greeted each other for about a half an hour, asking about everything except your family—that comes as just barely a trace at the end. So the other fellow said, "What are you doing here?" What are you doing with this character? What's he doing?" "Well," the son said, "you know, he's a nut, but he's harmless." [Laughter] The other guy looked very surprised; he didn't realize I understood by that time. He said, "What do you mean, he's a nut?" "Well," the sheik's son said, "he comes to us; he tells us he is looking for antique stones. So what do I do? I take him to the tels, the ancient-ruin hills, and he picks up something, and when he thinks I don't see him, he throws it away. Then he goes out into the deserts here and picks up rocks with his hammer." The other one shook his head, and the son said, "But he's harmless, you see." [Laughter]

ASPATURIAN: Was there anyone among the Bedouins who was interested in what you were really doing?

LOWENSTAM: There could have been. But since I lived with the number-one sheik, I had no way of finding out. I was with the top level. [Laughter] It was very interesting. The father of the sheik—a very old man—realized I had trouble, couldn't communicate, so he took me by the hand at the beginning. He would take me out and show me how to find out the rock, the sheep, something to eat and drink. I still had a few oranges with me, but when they ran out I had nothing to drink. He taught me to pick up certain tiny plants that one could hardly see. You chewed them and went all day without being thirsty. Occasionally we would hit a waterhole. I'll never forget that; you know, hygiene—how one is brought up. When we hit the waterhole, the horses, the camels, everything would go in. We would all take the handkerchief and drink the water through it.

ASPATURIAN: Did you do this, too?

LOWENSTAM: Yes. They didn't have handkerchiefs. I just used one myself.

ASPATURIAN: Were you segregated from the Bedouin women?

LOWENSTAM: Oh, yes.

ASPATURIAN: You were not permitted to talk to them?

LOWENSTAM: That's right. They were simply out of sight.

The one thing the district commissioner insisted on was, to the Bedouins I was a German. As a matter of fact, one day while we were in the camp, somebody behind me started to speak in Schwabian. I turned around, and I looked across, and all I saw was an Arab. They had brought in one from the German Templar colony, in the Haifa area, to check if I really was German. They didn't suspect that I was a Jew, but to them I was a foreigner.

ASPATURIAN: Did it excite you to be in Palestine because of what it was historically?

LOWENSTAM: Yes, certainly. It was exciting, and living with the natives all over was very exciting. Later I was in Jordan and unofficially in Saudi Arabia. It was unofficial because the British had officially confined me to Jerusalem. That happened because when I lived with the Bedouins, disturbances between the Arabs and Jews broke out. I didn't know about the disturbances, because it was perfectly peaceful where I was. But one day a British Army detachment appeared at the camp looking for me, dead or alive, because I was supposed to have been killed by the Bedouins.

ASPATURIAN: How did this rumor get started?

LOWENSTAM: My letters didn't go through. The runner who was supposed to take them somewhere apparently just threw them away after a while and then went home. I don't know why the British were interested in me. Through the Iraq Petrol Company, probably. [See Session 4, pp. 61-62, for elaboration on this point.] At any rate, the detachment grabbed me, and they did the craziest thing. Below the escarpment in the valley, there was the Khaduri school, an

agricultural Jewish school that had been evacuated. I didn't know of its existence. They dumped me there, where there was one British officer, and he was scared to death. He taught me how to handle rifles. We were standing guard day and night. [Laughter] This was to save me. I'll never forget that one night I heard something coming and I shot it, and it kept coming, and I shouted, and it stopped. The next morning, we found a dead ass. [Laughter]

ASPATURIAN: Were most of your relationships with the sabras, or with the British officials, or did you pretty much associate with both groups?

LOWENSTAM: All across. Some of my old friends, from childhood way back, were living in the *moshavim*, privately owned Jewish settlements in the Emek, and I was in contact with the kibbutz residents—the *kibbutzim*—and the Arabs. While I was mapping the Nazareth Mountains farther west, one day I came up the Mount of Precipitation, where the people of Nazareth were supposed to have threatened to throw Christ down. I came up and I thought I was seeing a mirage, because what I saw was this huge Persian carpet spread out and people sitting on it and people fanning people, and food all over. They were the Arab landowners who had come home from Monaco and Nice because they had run out of money. They were waiting for the peasants to bring in the harvest so they could take three-quarters of it to finance going back. They thought I was a big joke. They said, "You mean to say you are here voluntarily?" [Laughter] I said, "Well, in a way I am." And they said, "It doesn't make sense." They often invited me to their places. So I even got to know the old Arab gentry, who had sold land to the Jewish National Front.

ASPATURIAN: How were your relations with the British officials? What were your impressions of them?

LOWENSTAM: Most of them were narrow-minded, stupid almost. It often turned out as I got to know them better that they were either the third son—there was nothing left at home, so they were sent to the colonial army—or they were people who had done some minor thing that wasn't quite on the level and the family wanted to have them disappear. That was the bulk of those people—the lowest possible excuse to go. They were not interested in the country and the

people at all, with rare exceptions, like the district commissioner I knew. He had an interest in people in general, so he enjoyed what he was doing and he helped me a great deal.

While I was at the Khaduri school with the terrified officer, my notebooks and the collections I had made in that area were still in the Bedouin camps. The army detachment that had picked me up was like the harbor dregs of some British city, the worst people. You should have seen them and the way they talked. I couldn't get across to them that I had to get my collections back and take them with me. I knew I was in danger, because now the Bedouins were thinking I was a British spy, so I tried to get the officer to ride up and pick up my things in the camp. But he wouldn't go. But one day I noticed that the Bedouins had all moved north to what I knew was a central meeting ground. Apparently, as I found out later, they were having a gathering of the sheiks to decide whether or not to join the disturbances. When I saw that, I said to the officer, "I know how you can get up in the ravine and come right to the tent of the sheik I lived with, which was at the edge of the cliff. I know where my things are, if they are still there." I finally convinced him. We came up on two horses, and the Bedouin guard didn't know what to do, because he had no instructions. So I had time to see that nothing had been touched. I could identify the most critical collections from the bags' markings, and Arab horse saddles are very wide. I stuffed the material in, got my notebooks, and rode out. Then came another British detachment, and that had orders to take me to Jerusalem. So I came back to Jerusalem. I was not supposed to leave the city. They caught me a couple of times down at the Dead Sea and other places. In the meantime, I had joined the *Haganah*—the Jewish underground army. I lived in Talpiot, which was in those days a peninsula most likely to be attacked. My closest friend lived there, and his brother, a telephone lineman, was killed by the Arabs. There were other things going on. So at night I was in the *Haganah*. That was a tricky business, because after dark the British had imposed curfew. So we had our guns all taken apart and carried the pieces and slipped into our posts and assembled our weapons.

ASPATURIAN: What do you remember about the Jewish settlers at that time?

LOWENSTAM: I was most interested in the ones around the southern end of Lake Tiberias—the Sea of Galilee. They had lived there for almost a generation and a half. I met many of the leading people then, and they were quite interesting. They were unhappy about one thing. They

were proud of what they had achieved—all the orchards and the agricultural development—but they said their children were like big cows. They had no interest in literature, in music, in culture; they were just peasants. This intrigued me very much.

ASPATURIAN: How did you communicate with these people?

LOWENSTAM: Mostly in German—I didn't know much Hebrew—and French, and some English, but not very well.

ASPATURIAN: What were the feelings toward the Arabs among the Jewish settlers?

LOWENSTAM: They were very guarded, because there were attacks all the time. Usually, they had no personal relationships with them. When I lived in Ginnegar, in the kibbutz, there was a little Arab village to the east, which was on a terrace. For my geological mapping, I had to know what the terrace was made of. I never asked the kibbutz about that village; one day I just went in. The people there looked very astonished, and I couldn't understand it. After they recovered a little bit, I explained why I was there by pointing at what I wanted to do—that was before I lived among the Bedouins and knew Bedouin Arabic. Once they understood, they tried to get me to look at some caves. I knew that wasn't a very healthy situation, so I just checked; I knew what the cave wall rock was composed of and could determine the terrace's composition fairly rapidly. Then I left and came back. That evening, they asked me at the kibbutz, "Where were you today?" I told them I was in the Arab village; they didn't believe me. I said, "What do you mean?" "Why," they said, "if there's any thievery, if there's any shooting in this area, it's the people of that village. You want to tell us that you walked with a geology hammer into that village, and you came out without being scratched?" I said, "Yes. I had no idea." But apparently these Arab villagers were so taken aback that they must have assumed that there were others in the area. That's the only way I can explain it—that they didn't try to kill me.

ASPATURIAN: As far as you could tell, was this lack of contact between the Jews and the Arabs pretty much a general thing?

LOWENSTAM: Yes and no. Yes, in the sense that it was real; but no, in that it wasn't the Jews who tried to keep apart from the Arabs. They initially had Arab guards, but they killed and did other things. So then, the *kibbutzim* and the moshavs started having their own guards. That reminds me of an interesting thing. When I mapped the highest part of the Nazareth Mountains, there was a little Arab village. I knew the mayor of that town. I don't know how we communicated, but we communicated very well. One day he said to me, "I want to go down to the Emek at Esdralon Valley to the central town of Afula there and get something at the post office." So we came down on two donkeys, riding side by side. He had never been there. It was the first time he had ever needed the post office. The mountains were very austere—like the desert—and his eyes got bigger when he saw all the green, lush fields in the valley. He said, "Who owns that?"

"*Kibbutzim*."

"Who are they?"

I said, "Well, those Jewish people." He said, "Think what we lost." I said, "Now wait a minute. You see over there, that's an Arab cemetery. The natives—and there were very few Arabs living here—died from malaria. And then the first generation of the Jewish settlers who drained the swamps died from malaria." That didn't impress him at all. He said, "But look what's here!" In his mind, what was there had nothing to do with what it was before. His attitude was, "We don't have it; we live in the mountains." He could only see what was there now, and that's what counted. That really impressed me.

Also, when I lived among the Bedouins, the British high commissioner had made a speech, just after the disturbances had broken out. A runner came and reported on his inauguration speech. It sounded to me like a great speech. He tried to say, "Look, we are thrown together here—Jews, Arabs, and British. Let's make the best we can make, instead of fighting and killing each other and making it difficult. Maybe we can help everybody in some way." And those sheiks were just rolling on the floor, holding their sides, laughing. I said, "What's so funny about it? That's a very sensible thing." They looked at me like I was loco, and one of them said, "I'll tell you a little story"—that's how they would always answer. "The Turks were here, occupied this country. One day a Turkish soldier was killed during my father's reign. What did the Turks do? Nothing. In half a year, a Turkish regiment came. The chief officer was quartered with my father, and so on down the line, down the social ladder of the

Bedouins. They had a wonderful time, slaughtering sheep, having big parties. And suddenly, we realized, 'If this goes on, they will destroy our livelihood. We will starve to death.' So we got quietly together, my father and his buddies, and decided to turn over the man who had killed the Turkish soldier." He said, "You know, the Turks had an awful time finding a tree, but they found one. They strung that guy up and marched off." He said, "You see, Britain is weak. Britain is talking." And that's the whole story today, ever since. Only power and might is important. That's the sad part of it.

ASPATURIAN: Do you recall how the Jewish settlers felt about the mandate?

LOWENSTAM: It was of no concern. Life was so hard, to just survive, to make the daily ends. They had very little to eat. I told you how there was the deep mud, and how they tried to plant things and build things. An interesting thing was that the so-called top-level Zionists in the kibbutz were just complaining and talking. It was the others, who had drifted in from Germany and Austria and other areas, who were really doing the tough work. At that time, the Zionists were really looked down on—Zionism was all right for the salon but not for the daily life of existence. It was really interesting, because many of the younger people were not Zionists at all. They came there; they took it for what it was and tried to survive.

ASPATURIAN: Do you recall any discussion about the situation in Europe?

LOWENSTAM: No. The concern was more that there might be war in the Mideast, because, you see, Haile Selassie was in Jerusalem. As a matter of fact, when I was confined to Jerusalem, I lived a block away from Haile Selassie and his court, and I went with him and his prime minister and several ministers every day on the bus to town. He never saw anybody. He looked through people. So the situation was very volatile. It looked like a British-Italian war, initially, in that part of the world. It looked like it could be any time, so we were more concerned about the local situation.

ASPATURIAN: What did Jerusalem look like in those days?

LOWENSTAM: It was the old city, and then there were a few streets, the adjacent part of Jewish Jerusalem. It was very simple. I tried several times last year when I was there—I go every other year to the Weizmann Institute in Jerusalem—to locate where I lived in Talpiot. I was finally told by somebody who knew the area that all of that had been razed, and there are modern apartment buildings and more expensive villas substituted for the simple buildings. We had a very small, simple house, where I stayed with my friends.

ASPATURIAN: Did you ever think that you might want to just stay there, not go back?

LOWENSTAM: Initially I was thinking about whether I might want to stay at Hebrew University. But then the assistant of Picard pointed out that with Picard it would be a miserable life if you were under his thumb. He was right. It would have been very tough. So I gave up the idea then, toward the end.

ASPATURIAN: Did you meet any of the other faculty or intellectual leaders of the area?

LOWENSTAM: I met some in connection with a petition that the president of Hebrew University wrote calling for Jews and Arabs to live together in peace. There were problems at that time, not only with the Arabs, but with people in Talpiot who were Jabotinsky boys—members of the *Irgun*. I was one of several hundred people who signed that petition.

ASPATURIAN: From what you're saying, I get the sense that the Arabs were seen as the main challenge and the British weren't taken into account much at all in that period. Would that be correct?

LOWENSTAM: Yes and no. The British were a great nuisance, because if they caught anybody, then there was lots of trouble. Like when they caught me, just going out of Jerusalem when I wasn't supposed to. After the second time that happened, they really kept an eye on me. That's when I left the country. This was in mid-1936.

ASPATURIAN: Was it hard for you to make the decision to go back to Germany?

LOWENSTAM: Not really, because I wanted to get my degree and leave. I knew I wouldn't come back at this stage, and that my fiancée and I were going to go to America.

ASPATURIAN: In Palestine, did you have any associations with people in archeology or anthropology?

LOWENSTAM: Yes. The French consul, for instance, was an anthropologist from the Sorbonne. I don't know how I met him, but whenever I came to Jerusalem from the country—and also from Jordan, later, for a short time—they would always have me over and discuss things, because I knew Sukenik, the Jewish archeologist who got all my collections. The French consul was a wonderful man; I enjoyed him. One day while I was at his villa, he said, "Keep an eye out when you go around for any likely places where there might have been human habitation, with skeletal remains." So when I mapped the cave area at Jebel Qafzeh, near Nazareth, I often had lunch in the cave entrance, because there were no trees. That was the only place in the heat to get some shade and to rest for a short time. I would pick up stone implements in some areas, scrapers in others. When I told the consul, he wanted to see them, and then to keep them. That was the last I heard of him until after the war, when I got a monograph from the Sorbonne describing how he had excavated the Qafzeh cave in the Nazareth Mountains and found a Neanderthal woman skeleton with a stillborn baby skeleton next to it.

Also, when I lived with the Bedouins, there was a cliff facing the east. That region was all lava fields from the outpouring of the Rift Valley. The cliff was fifty percent limestone and fifty percent flint, and the whole slope was literally strewn with human artifacts. So in the evening, when I rode back to camp, I would usually stop for fifteen or twenty minutes before it got dark and pick up some of the implements. I remember I brought one back to Sukenik that wasn't complete. He took one look at it and said, "You see, these kinds of markings are Paleolithic; these were done by a Mesolithic apprentice, and these were done by a Neolithic apprentice. They would use the broken pieces, which weren't complete, to practice the technique of making implements." I had a fantastic collection of them. There was also a famous British woman archeologist who excavated old Jericho, and one time Sukenik said, "Wouldn't you like to see that?" So a number of us, including Picard, the geologist, came along. We came into a fenced

area where there was a sign, and it said, "Nothing must be taken from this site." The archeologist looked at me, and said, "You see that sign? You are here for the first time?" I said, "Yes," and she laughed and said, "If you find something, you can keep it." And Sukenik said, "Well, I heard you." We walked along, and I suddenly saw out of the corner of my eyes some reflections on the ground. There was a little piece of flint sticking out. I started to dig, and I got a beautiful Mesolithic scraper. You should have seen her expression! I was proud to have found something, so of course I kept it.

Then I got back to Jerusalem. The next day, Dr. Wilson, from the Rockefeller Museum, called me up, asking if I would like to have lunch with him. [Laughter] I said, "Does the news travel this fast?" He said, "Yes, it does. Do you want to have lunch with me?" I said, "OK." [Laughter] So, sure enough, he said, "You shouldn't keep it. All the Jericho stuff is in England; nothing is here. This is the first piece that ever got out intact." I left it, of course. It wasn't very hard. I somehow always felt these people had the right to it. I had also found the most beautiful Maccabean coin in the Nazareth Mountains. When I showed it to Sukenik, he grabbed it—the best they ever got, [he said]. I understand that it still is.

ASPATURIAN: Did you ever meet Ben Gurion, or Weizmann, or any of those people?

LOWENSTAM: I met Ben Gurion in a group at Bet Alpha, at Lake Tiberias, one of those famous *kibbutzim* where they talked about their children being like cows. [Laughter] He came in and he shook hands with everybody. Then they had something to discuss. But there were so many of the famous old-timers there. I forget the names. They were really interesting people. We talked about philosophy and other things, but not about the political situation.

I remember one other thing that was really intriguing. When I worked at the highest, very scenic, tiny village at the top of the mountains, at lunch, we would sit down. The men and my guide would sit down with me. The peasants who were plowing would sit down. But the women had to go on plowing; they didn't count. Suddenly one day an old blind man came down, guided by some kid. He heard me talk. You know, in the mountains I wasn't supposed to talk—a foreigner. And they didn't know I was Jewish. Suddenly he got all excited, and he had a vision. He said, "This is the end. This land will be taken over by the Jews." That was in '36. They thought he was out of his head, but he went on and on.

ASPATURIAN: What were your thoughts, as you heard him say all of this? Was it translated for you?

LOWENSTAM: It was translated. I thought he was rather off. It never occurred to me that this part would be taken over. I never thought in terms of the state. I knew certain areas would be Jewish. But this was very eerie.

ASPATURIAN: When you decided to go back, did you have problems getting back into Germany?

LOWENSTAM: No, not at all. As I told you, Bavarian Munich was pretty good. Farther north, like Nuremberg, was terrible. But in southern Germany they were not as excited about Hitler as the Prussians.

ASPATURIAN: Did you go straight back to Munich?

LOWENSTAM: Yes.

ASPATURIAN: Had the situation changed in your absence?

LOWENSTAM: No, not in Munich. In that group at the university and in Schwabing, where I lived, they were very sympathetic to me. I remember the proprietor of something asked me if he could take some of the fossils and rock pieces I had brought back from Palestine and use them for a nativity scene. So of course I gave them to him.

ASPATURIAN: How about in your parents' hometown? What was the situation there?

LOWENSTAM: It was grim; it was bad.

ASPATURIAN: What had happened to your sister? Was she able to attend school?

LOWENSTAM: She went only up to a certain stage. She learned a trade, like a secretary. Then she got married. She left for Brazil before I left for America.

ASPATURIAN: What made her choose Brazil?

LOWENSTAM: We had family there.

ASPATURIAN: Was this your step-grandfather's family?

LOWENSTAM: Yes. Everybody went to Brazil, except me.

ASPATURIAN: Was that difficult, arranging to go to Brazil?

LOWENSTAM: No. Her husband was very well-to-do. In South America, in general, if you bribed a consul with money, the doors opened. So she had no difficulty and left early. My parents hadn't left yet. They were rather unhappy that I was going to get married and that we would leave for the U.S. I think they didn't know how to get out at that time. They finally left in 1939.

I came back the middle of '36 and spent not quite a year finishing my thesis. I worked pretty fast and I got things done, and the thesis was accepted. But, as I told you—

ASPATURIAN: You were not able to win the doctorate but your professors wrote a letter on your behalf.

LOWENSTAM: Right.

ASPATURIAN: When and why did you decide to go to the University of Chicago?

LOWENSTAM: My fiancée and I got married in 1937, just before we left for America. My wife's uncle asked us to come. We went to Chicago also because the aunt wrote that she knew people at the university and there'd be no problem to get in. It turned out she had no idea. These

relatives were selling stockings and underwear for ladies at two stores in Chicago. The aunt sold to the wives of faculty at Chicago, but I don't think she ever talked with them.

Before we left, the Gestapo came and went through all our books. They spent half a day going on, page by page. I assume they were looking for money. They didn't find anything. Then they saw my mediocre coin collection, so they took that. As I told you, I didn't feel bad about it at all. There were no Maccabean coins left in it, but there were Roman coins, Greek coins, Crusader coins, Arab coins, and Ubek coins that were coined over by the Arabs with Koran verses when they took over again, and vice versa. It was historically very interesting.

I have to tell you an interesting story—how one survives sometimes. Before I left, we had something like 2,000 or 3,000 marks left, maybe more. We were allowed to take all of our belongings but no money. After we had paid for everything in advance, all we could take with us was ten dollars equivalent. I decided, "I'm going to get a petrographic microscope." That cost about 3,000 marks. I went to Giessen, where I had a great uncle and great aunt who had a factory. They disappeared during the war and I never saw them again. Later, when my sister looked into it, she found they had been deported to the camps. But in those days they were living next to Leitz, and Leitz made petrographic microscopes. I went to my great uncle and I said, "What are the chances of getting a microscope here?" He said, "I know the owner very well; we can talk about it." So we went over. The owner looked at me and said, "No. We can't let you have one." I said, "Why?" He said, "There was just an international meeting in Berlin on ore petrology and the Russians ordered a thousand ore microscopes." Hitler had ordered that no domestic microscope was to be built during that time until the Russians got theirs. So while he was ranting in public about the Russians, he was secretly reserving these microscopes for them. It was because they were paying in gold. The owner said, "When do you need it?" [Laughter] I said, "Well, I'm leaving." He said, "OK." He got it to me.

ASPATURIAN: How did you get it out of the country?

LOWENSTAM: It was part of my belongings. Those local Nazis didn't know what Hitler had ordered in Berlin.

So we just left. We were to go by train to Holland, take the ferry to Harwich, I think, and then to London to stay two nights, and then to Southampton, on the *Aquitania*, to New York.

ASPATURIAN: You mentioned after our first interview, when the tape was off, something about leaving as a Polish citizen.

LOWENSTAM: Oh, yes. [Laughter] To get a visa to come to the United States, we had to go to Stuttgart—that was the closest American consulate. We came in, and my wife got hers in no time. I was sitting and sitting there. I was called back to have another chest X-ray. Finally, the consul called me in, handed me the passport, and I opened it, and I see in the German passport a Polish visa. So, like a crazy guy brought up in Germany, I said, "There's something wrong. I'm not from Poland." He looked at me and said, "Young man, you don't realize it; I have something like three Polish emergency visas, and I just gave you one. So be glad you have it." I said, "But why did you give me a Polish visa? I was born in Germany; I live in Germany. I never was in Poland." He said, "Look, you see that map over there. Where is your birthplace? Show me." So I showed him. He said, "What country is it?" I said, "Poland." He said, "You see, we don't worry about what a town was when you were born and when you lived there. We look at where that town is today. And today it's in Poland; therefore, you have to have a Polish visa." If I had known beforehand that I had to get a Polish visa, I would have never planned to go to the States, because for about ten to fifteen years, the quota had been overdrawn. So I was just fortunate—it was really a miracle that I got it. [Laughter]

I also remember that before I left, I had to have a statement that I was exempt from military service. I got it in Beuthen. A police commissioner there had to give it to me. I came in and he said, "Where are you going?" I said, "Chicago." He said, "Aren't you afraid of the gangsters there?" And I said, "No. I don't know anything about it." "Well," he said, "they shoot people there." As we left, he gave me the stamp that I was exempt, and just before he opened the door, he looked at me and said, "I wish I could go with you." So they were decent. Many of these people used to be very anti-Semitic before the Nazi period. They were German National Party members who felt cheated because of what happened to their leader, [Alfred] Hugenberg, the famous publisher. He tried to use the Nazis to get into power and then take over, but instead he disappeared. After that, his supporters took a totally different attitude—quiet. Like when the commissioner said, "I wish I could go with you."

ASPATURIAN: Do you recall any other incidents like that?

LOWENSTAM: I can't think of any. But in general, it was mostly that I couldn't walk on the street; my wife was afraid to walk with me on the street because I looked so fierce. I was so mad, seeing those Nazis, that she was afraid they would start to grab me.

The only other thing I wanted to tell you is a very interesting story. We came to London and spent the day there. And I ran into an old friend, Ernst Philip. He was a Jewish mining engineer—which was very unusual in those days—because his uncle was a billionaire who owned all the tin mines in Peru. As we said goodbye, he gave me his address, and for some reason his telephone number, too. And just the evening before we were to go to Southampton we found out the *Aquitania* was delayed for a week. After we had paid for two days at the hotel, we had nothing but ten dollars equivalent in our pockets and we couldn't see how we would manage. I was desperate, and I finally got hold of Ernst Philip and said, "Look, I hate to bother you, but here's the situation. I don't know what to do." He said, "Well, it's rather difficult. I'll see what I can do." The next morning, when we were supposed to leave the hotel, there was under the door a letter. I pulled it out; it was from Ernst Philip. It said he had to leave to go somewhere but that there would be a car coming, picking us up and taking us to Hampstead to the house of his billionaire uncle, who was with his wife on a trip around the world. We would be taken care of while we were there. I'll never forget this. That night we found ourselves sitting at a fantastically fancy, big table with five, six servants taking care of us, and here we were—paupers. [Laughter] I almost laughed when I thought about the irony of the situation. So that's how our new life began.

HEINZ LOWENSTAM

SESSION 4

July 12, 1988

Begin Tape 4, Side 1

ASPATURIAN: At the time you left Germany, what was the status of paleontology as a discipline within geology, and in general? Did it have respectability as a separate discipline?

LOWENSTAM: It depended mostly on the people. For instance, Munich was very famous for its paleontology, and people came from all over the world to study there, so it had quite a standing at the university, too. Another big name was Schindewolf, who got those crinoids for me in the Prussian Geological Survey, which he didn't have to do—dug them out from all the dirty places. When he took over Tübingen, Tübingen became a well-known place for paleontology. It was the same way in this country. Vertebrate paleontologists were particularly respected, because dinosaurs and giant mammals are always spectacular to the public.

ASPATURIAN: What about the field you were especially interested in—marine ecology?

LOWENSTAM: That didn't really exist, initially. When I studied in Germany, one of the few people interested in it was Rudolf Richter in Frankfurt, the one who sent me to Munich and who didn't want us to stay at Frankfurt because of the palace revolution. He founded Senckenberg-am-Mer, on the North Sea coast, and made observations of recent forms in comparison to fossils, and then started to interpret some of the fossil record in these terms. But in general it was mostly fantasy.

ASPATURIAN: When you got to the United States from England, did you go immediately to Chicago?

LOWENSTAM: No, I stopped off first in New York. The company of Ernst Philip's uncle—whose palatial place we stayed at in London—was in New York. They called and told me I should work for them in the Peruvian tin mines. But I went on to Chicago. And that was, at

first, a real disaster. As I told you already, this aunt of my former wife had given us the impression that it was very easy to get into the university. Actually, nothing happened at the beginning. We arrived in June, and it was vacation at the university. Also it was still Depression days, and I had to try to find a job. I went to a friend of my father's brother who had a carton factory, to find out if he could use me in the factory. He looked me over and apparently decided I wasn't good for it—probably would gum up the operation. [Laughter] But after he found out where I stood, practically a doctor's degree and no doctor's degree, he said, "I'll tell you something. My son has formed a group of young well-to-do men who are supporting people of your type. If you are accepted at the University of Chicago, they will see to it that you get some financial help for living expenses.

ASPATURIAN: Did they know you were coming at Chicago? Had you had any contact with faculty members there?

LOWENSTAM: None.

ASPATURIAN: How did you go about gaining admission to graduate standing?

LOWENSTAM: First of all, I had to find out from Edgar Goldstein, the son of the carton manufacturer, if his group would really help me. I went to him, and he was a very nice person. He was a theatrical agent in Chicago, and apparently in quite high standing, because Greta Garbo would come through and would stay at his place to hide from the public, and so would all kinds of other famous actors and actresses. So he must have been quite a well-known person in the field. He listened to me and he said, "Just go over and find out, and if they say they accept you, we can do something for you."

So I went to the university—to the geology department. It was late June by that time, and school was over, and when I saw a man walking around geology in a lab coat, I assumed he was a technician. I tried to find out from him how to find out if I could possibly be accepted as a student. He realized I didn't know much English, so he said, "We can speak German." He turned out to be Professor Johannson, of Scandinavian origin. He had written three volumes on petrology, was very well known for it, and I still didn't realize who he was. I thought he was an

assistant. When he said, "Come to my office," I looked around, and looked at Johannson all over, until I realized I knew him from Germany. I asked him, and he said, "Yes, I am just retired; I'm leaving, getting rid of everything." I said, "What are you going to do?" He said, "I always wanted to write dime novels, and now I'm going to do it." And he became quite famous for it. [Laughter]

He said, "Well, nobody is here right now. You'll have to wait a month, and then you can see the geology chairman, and I will see to it that he's alerted to it. And the chairman of paleontology—Professor Carey Croneis." In the meantime, my wife's aunt got very unhappy about the fact that we were just sitting around. We were not living with her, though. We had been sent some money through an uncle who had a Swiss account, so she and her husband were very relieved that we were not a financial burden to them.

ASPATURIAN: But she felt you should be working.

LOWENSTAM: Yes. For some reason she still thought I was in a very lucrative and important business, so she took me to some counselor in Chicago to find out what my possibilities were in the future. He looked at her, "A geologist? There are so many, we plaster the streets with them." So as far as she was concerned, I became nonexistent, because she had thought I was a very important person up to this point. [Laughter]

But I came to the university; I talked to them; I showed them all my grade sheets, and they went through them.

ASPATURIAN: And the letter, I assume.

LOWENSTAM: I hadn't thought of it. The letter happened to be in my pile of papers. They saw that letter and said, "Could we open it?" They read it, and—I'll never forget—their eyes got big. "Broili, Dacqué, they recommended?" After that, the whole atmosphere changed dramatically. I immediately got a scholarship, in the middle of the year. I was told I would have to take a few courses, translate my thesis, and within a year or two I could get my degree. That's when I realized how important that letter was. It was a miracle that Broili and Dacqué had done it, too, because without it, I don't know. As it turned out later, the head of paleontology at Chicago was

very anti-Semitic. Not only that, but there were almost no Jews in geology in those days. There was only one, Larry Sloss, who finally got a job as a lecturer at Northwestern. Before the war, it was a closed profession, apparently.

ASPATURIAN: What were your initial impressions of Chicago and of scientific training in the United States?

LOWENSTAM: Chicago was rather bewildering, because everything was different. At the university, I remember being rather taken aback when I started courses, because we would have quizzes. The last time I had had a quiz was in high school, and I was startled that one would have that kind of treatment.

One interesting thing was that the successor of this petrologist, Johannson, I just mentioned, was N. L. Bowen. He had just come from the Carnegie Institution. In the textbooks I had used in Germany, there was exactly one phase diagram—the only quantitative thing in the whole textbook—which I had always admired. That was his work on high-pressure, high-temperature effects on mineral formations in deep-seated rocks. He was unique, and although he was totally out of my field—he was professor of petrology—I got interested in him. I enjoyed the quantitative approach, and he was interested in me because he had been on the East African rift expedition with Bailey Willis and had studied volcanic rocks along the rift. Since the area I was working on in the Mideast—I had been in Syria, too—had samples of those, he thought it would be a good idea if I did a research project on the petrology of these basaltic rocks.

ASPATURIAN: In addition to translating your thesis?

LOWENSTAM: Yes. But he didn't say I must do it. He said it would be interesting. He was interested, and I enjoyed it. I did a study on that, which came in handy later in a very curious way. The head of the Oriental Institute in Chicago, which had mostly done work in Egypt and Palestine, had, among others, excavated the old Megiddo fortress in the Emek of Israel. In ancient times it was occupied repeatedly by whoever the great powers were from one century to the next. King Solomon occupied it; so did the Babylonians. The period the Oriental Institute was looking at was the Egyptian occupation. Among other things, they had found a number of

clearly Egyptian statues in the area, and the question they raised was, How big was the Egyptian colony? Did it have only military men, or did it also have, by any chance, a supporting group of civilians, including artists? To find that out, they needed to know whether statues they had found had been hand-carved in Egypt and brought there, or whether they were carved locally. So they asked me to look at the rock, to try to determine its origin. I was allowed to take small chips with me.

The big joke was, just shortly before I left Palestine in 1936, I had been around Megiddo, and I had noticed a big basaltic plug. It was very fresh rock, because they had used it for quarrying and making a road. I had taken some samples of that basalt back to Germany and on to Chicago with me, because they were unique in their crystal structure. One of the samples the museum gave me was made of that same basalt. So I knew that the statue it had come from was made locally. There was another chip from a statue that also looked like it was from local rock, but it was not as clear-cut a case as the first one. In the first case, there was absolutely no question. The rest were Sinai-type rocks, so they could have been taken to Egypt and made into statues there, or by artists going into the Sinai, picking up the material, and making it into artifacts there. But at least it established that there were artists in military colonies.

Bowen was helpful to me in other ways, too. One day while I was preparing for my doctorate, Bailey Willis, the famous American tectonics man at Berkeley, came in. He had brought this tome on the Rift Valley in East Africa and had written a paper about the Dead Sea area. He interpreted the rift as a ramp valley. My work in Palestine didn't support his theories about the geology, so Chamberlin, the editor of the *Journal of Geology* who was a professor at Chicago, called me in to see Willis, and said, "I give you an hour. You might want to talk with him. He would be interested to hear what you have to say." So Bailey Willis grabbed me. When he heard that I didn't believe in his analysis—I was very careful about what I said to him—he took me down to the geology map and showed me all kinds of things and tried to woo me to change my mind. I didn't know what to do. I remember I was very confused. I walked up to my office afterward and ran into Bowen, who said, "What's the matter?" I told him, and he said, "Come into my office." He closed the door and said, "I'll tell you a secret. Bailey and I went on the East African Rift Valley expedition together. His monograph was written before he ever set eyes on the rift. Does that solve your problem?" [Laughter]

ASPATURIAN: You mentioned tectonics. Was plate tectonics theory accepted in those days?

LOWENSTAM: Oh, no. I told you how I had heard Alfred Wegener lecture about the movement of the continents as a boy and was convinced from there on, but the view in America was very different. When I came to this country, continental drift didn't exist. T. C. Chamberlin—the father of one of my professors at Chicago and a very famous geologist—had pronounced "the permanency of the continents and ocean basins." There was no space for continental drift. Once, in a seminar with [R. T.] Chamberlin the son, I tried to interpret something in terms of continental drift. I still remember him—he was six foot five—towering over me and saying, "Young man, in this country, we believe in the permanency of the continents and ocean basins. Are there any further questions?" [Laughter]

ASPATURIAN: What was the attitude at Munich when you were a student there?

LOWENSTAM: They accepted it. When I got to Caltech, Beno Gutenberg, the seismologist, always egged me on to do something with him to get the American geologists out of the rut they were in about it. But somehow it never came off.

ASPATURIAN: How did you find the social and intellectual climate at the University of Chicago compared to that at the University of Munich?

LOWENSTAM: It was not as broad as Munich, not in those days. On the other hand, I should be careful in making such a statement, for the simple reason that my English often wasn't good enough to pick up a great deal. I would not attend as many lectures outside of my own field as I did in Munich, because I was preoccupied just keeping my head above water. I went to hear some of the biologists, like A. Carlson and others. In the cultural area, we had a small circle of refugees, intellectuals. One was a writer who wrote for the Schoken Publishing House. He had a kind of seminar-type of affair on Jewish philosophy, which was very interesting. He always tried to convince me and couldn't quite get me into thinking along the lines he was, but I found it intriguing. There were also people like Dr. [Helmut Paul George] Seckel—a physician from Berlin. He was not Jewish, but his wife was. His father was a supreme court judge in Berlin.

He had left as soon as the Nazis came to power, because he wasn't going to subject his wife to the Nazi kind of atmosphere. He had a hard time as a German. He was lost, in a way. But it was very nice. We were very close. He had many original pictures, like some Liebermans and Cezannes, and he would loan them to us so that we would have something to look at. He also tried to help me get an affidavit for my wife's niece's parents when a brother of hers wouldn't do it. But we got the papers too late, and so they went to Auschwitz. But he tried.

ASPATURIAN: Were you in communication with your family back in Germany during this period? Were you able to receive or send letters?

LOWENSTAM: Yes, we occasionally got a letter.

ASPATURIAN: Did you know, before they left, that your parents had in fact succeeded in getting permission to emigrate?

LOWENSTAM: No, they never mentioned it. I found it out through my sister in Rio. She wrote to me that my parents had arrived.

ASPATURIAN: How did they finally get out of the country?

LOWENSTAM: They went out on the last ship that left Hamburg—a German ship—before the outbreak of the war. When they reached the English Channel, the ship was called back to Hamburg. Either the war had broken out, or it had become too dangerous to go on that voyage. We heard about it—I don't know how we found out. We thought they were lost. Then the next thing we heard was that they came down to Naples and left their belongings there in the good hands of the Fascists—Mussolini's boys—with the promise that nothing would happen to them, and they would get them back after the war. Of course they never saw their luggage again. But they miraculously escaped and went on to Rio by ship. I never found out what made it possible for them to go from Hamburg to Naples. When I asked, my father either changed the subject or forgot or wanted to tell me something else. I never found out. It must have been some kind of underground railroad running out of Germany.

ASPATURIAN: While you were at Chicago, did this Professor Bowen serve as your adviser, or did you have another adviser?

LOWENSTAM: No, my adviser was the paleontologist, Professor Croneis—the anti-Semitic one I told you about. One day I happened to be in the Walker Museum, where he had his office. He was in his office with a fellow student of mine who was talking very loudly. He was saying, "Now, here Heinz comes from Germany, escaping the Nazis, and you treat him basically the same way as the Nazis. What do you think he will think about America?" He went really after him—I believe he's now at UCLA. I had been puzzled about Croneis's attitude toward me—he was very cold and reserved—but I didn't realize why, until that happened. So I became very distant and only did what I had to do with him. I graduated in '39, but first he had to make a speech—that was in '38. He told me one day—and that's how I really found out—"It's very hard to get a job at a university for a Jew, because there are none in paleontology. There is Larry Sloss, who was one of my students, the only one in the United States; and he's now at Northwestern. Furthermore, it's not easy to do something for Jews, you know. Look across the quadrangle. You see chemistry over there. That used to be an all-Gentile department. And then Professor So-and-so came in and became chairman; and look now, sixty percent of them are Jews." I got very annoyed with that.

But then, something surprising happened. While I was at Chicago, every so often oil companies would send people over to inquire about students who were interested in jobs. We had to sign up, and I didn't—I was never interested. But after Croneis acted like that, I went over and said, "Yes, I want to be included." That's when I met the chief geologist of Socony Vacuum. The department chairman, who was a very old-fashioned character, took my thesis to the company geologist. Later the chairman told me what happened. The geologist took one look at the title and said, "Oh, that's old stuff. I don't want to see it."

"Is anything wrong, Sir?"

The geologist said, "Oh, no. But we already have not only his notes from Palestine, we have the notes he made for our people in Jordan and in Saudi Arabia. He never knew that he was a leg man for us; we financed his work." So my friend in Munich—the one who published books of the Middle Ages—somehow had connections with the oil companies. Socony Vacuum

was a subsidiary of Iraq Petroleum Company. And that's how he financed my trip to Palestine. Up to that time I had never realized how he had done it. But it explained why I kept running into geophysicists and geophysical crews the whole time I was in the Middle East. They obviously knew where I was, too, which amazed me, and they were always so nice. When I was out in the field, they would say, "Take a look over in this region; take a look at the rocks." They were doing seismic work. I would say something like, "There's a fault over there," and they would say, "Yes, that's what we wanted to find out." And every so often, one of these company representatives would show up and say, "How would you like to take a trip to Jordan? To Saudi Arabia? We'll pay your way, if you'll write up a field report for us when you come back." I would always say, "Sure!" I took these trips all over the Arab world, and when I came back I would make them a copy of my notes. I was working for them without knowing it the whole time I was in the Middle East.

ASPATURIAN: When you found out about it, what happened? Did you go to work for them?

LOWENSTAM: No. They were only looking at prospective possibilities, if business got a little better. The petrol industry was in bad shape before the war. They said they would keep in touch, but I never heard.

The next thing that happened was just a few days before I graduated. The geology department chairman called me in and said, "You know, Pure Oil Company wants somebody to evaluate a job somebody else has done on a mineral map of Europe. Would you be interested in that? They would pay about a hundred dollars a week." In Depression days, that was a lot of money. He said, "You would just look over what they have. We are always interested in the geopolitics of their ore deposits." I laughed, and said I would do it. I went over, and Theron Wasson, the chief geologist, gave me everything that had been done so far. I sat down and looked it over, and after a short time he came back and said, "What do you think about it?" I hemmed and hawed, and he said, "I want the truth. That's why we have you here." I didn't say, "It stinks," but I said, "It's impossible; it's terrible." The whole thing didn't make sense the way they had handled it. I said, "Furthermore, we need more data." He looked at me and said, "Would you want to do it?" I said, "Yes. I'm just graduating, I have no job." So, to make the story short, I worked for three months for the oil company. I revised the whole study and fixed it

up into book form. They wanted my name on it, and I refused, because my parents were still in Germany and there would be more trouble from the Nazis if they got hold of a copy. It was supposedly only circulated to about 150 major companies on Wall Street, with big holdings. It was not on the open market. I really had to fight to keep my name off. They said, "You did it, so you should get credit for it." I said, "But you don't understand. If they find out, my parents could really get into bad trouble." They just didn't understand.

ASPATURIAN: Did you find this a general rule, that people in America simply did not grasp what the situation in Europe was?

LOWENSTAM: Not only there. After the Nazis came to power, my friend Ernst Philip—the young mining engineer in England who got me to stay at his uncle's place—wrote letters to me in Germany, and open cards that said, "What are those Nazis doing? I don't understand it. Explain that to me." After a while, when I didn't answer him, he wrote a postcard, "Not worthwhile corresponding with you because you don't answer my questions." That was the general attitude.

Wasson tried to hire me for the company, but I said, "No." He said, "What do you want to do?" I said, "Well, I like to study fossils." Not long after that, he went down to Texas. There were three universities that the oil companies there and in Louisiana financially supported, because they had oil fields in those areas. He tried to find out if he could get me into one of their geology departments. But not only were there no jobs, a Jew was an impossibility. He came back and said after a while, "I'm sorry. I tried my best. Are you sure you don't want to join the company?" I said, "No," and he said, "That's it."

ASPATURIAN: How did you find the job at the museum?

LOWENSTAM: Within a week after I quit the oil company, Professor Croneis called me in and said, "I've found a job for you. It's at the Illinois State Museum as a curator." I said, "Oh, no, you wouldn't want a Jew to be in a place like that." And he looked at me, and he said, "Forget it!" I said, "Well, isn't that what you have been trying to tell me?" He said, "For Christ's sake, stop this stuff!" So he got me the job at the Illinois State Museum.

HEINZ LOWENSTAM

SESSION 5

July 14, 1988

Begin Tape 5, Side 1

ASPATURIAN: When we finished last time, you had just been hired as the curator of the Illinois State Museum, somewhat to your surprise. What did your duties there entail? What was your job?

LOWENSTAM: I was to have exhibits in the museum on fossils. I was to teach in the evenings in the school of adult education, sponsored by the museum. Later, when I became an enemy alien, that became very important. [Laughter] Those were the main jobs. The director, Thorne Deuel, was an old military man who had retired as a result of being a colonel or something like that. He had gone to the University of Chicago and got the doctor's degree in anthropology and archeology and was also research-oriented. When I asked him if I could do some research on the Silurian fossils in the Chicago area and surrounding area, he said by all means, and they would publish it. So, that was my job.

But I started out cataloging fossils. The Illinois State Museum was a very famous institution in the 1870s and 1880s, because a man by the name of [Amos] Worthen was state geologist and he was located in the museum. He had amassed an enormous amount of fossils from Illinois and had published a series on the state's paleontology. He became world famous. I found that the library there, which nobody ever used, was full of his exchanges with paleontologists all over the world. The rarest things I finally salvaged. But my main purpose initially was to identify fossils.

Those were the days of the Depression, so we had a big WPA [Works Progress Administration] crew at the museum. One of their jobs was to clean Worthen's old fossil collections. There was dust half an inch thick on them, I've been told, because nobody had ever cleaned them before. The only other curator who ever was there had brought in the mineral collection of Illinois and was not interested in fossils. Those WPA people had a supervisor who was very good. He saw that there were labels on each specimen, and he asked the people who cleaned the specimens to carefully remove the labels and put rubber bands around them and

stack them in the same drawer, which meant that you couldn't tell what label belonged to which fossil. There were lots of fossils called types—a specimen that was described for the first time and everything after that referred to it. It was important to determine how many of Worthen's types were still in existence. And here were these drawers full of freshly cleaned fossils, with all their labels neatly bundled into packages with rubber bands around them!

So I went through Worthen's manuscripts—I could find some of those—and his descriptions and publications, and I located about fifty or sixty percent of his original material. That was my initial job. After that, I was asked to do exhibits and play around with the experiments and teach.

The story of why I asked Dr. Deuel, the museum director, to allow me to do research on the Silurian fossils was a very curious one. It went back to the days when I was a student in Chicago and wanted to look at outcrops of fossils. I didn't have any money to go on trips to do that, so I discovered a streetcar that went on the South Side, way over to Stony Island. It was called Stony Island because there were fossil reefs cropping out—coral reefs. I went over there and it was in terrible shape. Then I discovered next to it long dump piles that had been made when drainage canals were built to connect the Illinois River with the Great Lakes to get barges through. In digging the canals, the work crews had dumped all this stuff on the side. I started to walk over those old dump piles and I found very nice fossils, all marine, and I knew they were from the Silurian period—about 400 million years old. Some people from the Field Museum had already cataloged some of these fossils—the black shale type—and had made soft-bodied impressions of them. But nobody had looked at the skeletal remains in dolomite. I made a big collection of that material over a period of time and then tried to identify it. I couldn't. I researched the local literature and none of the fossil groups that had been described from the Chicago area fit what I had found at all. So one day while I was still a student, the curator at the Walker Museum of the University of Chicago came in while I was working on this problem. I said, "I don't understand it." He said, "You know,"—he was a bibliophile—"I have some old monographs of the paleontology of Tennessee and various areas. Take a look at those." To my surprise, I found I could identify everything as being of the same type as the Tennessee stuff; often the same species but not always.

ASPATURIAN: What did that mean?

LOWENSTAM: To me, it was rather surprising and I didn't quite catch the significance. A few weeks later, I ran across a textbook on the geology and paleontology of North America by [Charles] Schuchert. It said that during the Silurian period there existed a northern province that included the Great Lakes area and a southern province that included what is now Tennessee, Kentucky, and so on. Because the fossils from these two regions were totally unlike, it said there must have been a barrier between them. The northern ones were related to European stuff that had been previously identified as originating in Scandinavia and Bohemia. So the northern ones came to North America via a northern route and the southern ones by an entirely different southern route. But there was another paleontologist, who was vociferous and imaginative, at the National Museum of Natural History—[Edward Oscar] Ulrich—who always disagreed. As soon as Schuchert said one thing, Ulrich said the opposite, or vice versa. That used to be very common in geology in those old days.

Ulrich said, "That's wrong. There was no barrier in between." His theory was that the continent teeter-tottered. When it tilted to the north, the northern forms came in, and when it tilted to the south, the southern forms came in. That meant that the two forms couldn't be time-contemporaneous. Well, I had southern fauna from the Silurian mixed in with northern fauna from the same period. Why was there just that little mixed patch there? Somehow, the southern fauna must have come into the north.

Well, my professor, Croneis, was like Broili. He only put his lederhosen on twice a year, when they had the field camp in Missouri, and maybe they had a picnic, with some colleagues. I told him about what I had found, and he said, "That's impossible, because the identity of the fauna is well established. Did you look at the textbooks?" I said I did. "Did you look at Ulrich?" I said I did. He put his lederhosen on and went out with me to the dump pile, because he was convinced that some southern hillbilly had moved in from Tennessee and had dumped the fossils in his backyard. But when, huffing and puffing, he went with me about two miles across these dump piles, and this thing continued, he had to admit that they were local specimens. So that was a mystery: How did they get there?

When I got to the Illinois State Museum, one of the first papers I published was to suggest that the barrier that Schuchert proposed between north and south did exist, but locally it sagged for a while. During that time, the water masses mixed, and larvae from the south could come

into the north. Then the barrier closed again.

ASPATURIAN: Did the paper get a lot of attention?

LOWENSTAM: No. I got only a letter from Schuchert thanking me for my interpretation, because it agreed with his theory. Ulrich was dead by this time, so he couldn't complain. [Ulrich died in 1944, outliving Schuchert by two years—ed.]. But I got to wondering. Dr. Slocum, who was a curator at the Field Museum, had also gone along the drainage canal in another area, and he had found also Silurian fossils. I recognized that they were of the same type. He only described them—they were younger than the ones I had. So when I came to the museum, I asked if I could go back and really study in the field, what's the relation of those fossils. Because dump piles didn't mean anything; you couldn't get to the strata that were under water. I wanted to see all the biozones. I started to go around, and wherever I went I would find more and more "southern fauna."

To make the story short, what I discovered was that the whole concept of Chicago's Silurian fauna had been hatched by people sitting behind their desks in the museums at Yale University and the National Museum. They had fossils from the region I was looking at there. If they came from the Chicago area, they were different from those in the south. Why were they different? Because they all came from the coral reefs. They were all reef organisms, and they were big. Nobody had looked at the small, little fossils in the shales that were between the reefs. But I did and I discovered that the two types coexisted there. The reefs had the so-called northern fauna, and the so-called southern fauna was the flat, level, bottom community of the open ocean's surroundings. And so I published a little book, ultimately in '48. [*Biostratigraphic Studies of the Niagaran Inter-Reef Formations in Northeastern Illinois.*] By that time I was at the Illinois Geological Survey, but it was published by the Illinois State Museum. I had the whole concept documented, the outcrop area. And for many years the professor at Yale, the successor to the first Silurian fauna theorists, wouldn't talk to me. [Laughter] This was the first paleoecologic study I made on a big scale.

ASPATURIAN: By this time, you'd been in America for about four years. Were there major differences in the European and American approach to paleontology that you were able to pick

up by this time?

LOWENSTAM: Yes. First, because my tutor in Europe was Dacqué. Dacqué introduced me to comparative functional morphology. Many of those fossils of the reefs were unique in their engineering structure, adapted to turbulent, high-turbulent water. So that helped tremendously to understand what was going on.

ASPATURIAN: What about the approaches to the field, as it was taught in the U.S. and taught in Europe?

LOWENSTAM: They were not interested here at the time in such paleoecologic questions.

ASPATURIAN: Were you viewed as a maverick at Chicago because of your interest?

LOWENSTAM: In a way, yes. But then I was also Jewish! [Laughter] And Deuel was very objective; he was very nice. And when it finally dawned that I had enough hard evidence to back up my theories about the reef and that that was the answer, he enjoyed it.

Also while I was at Chicago, Bryan Patterson, a vertebrate paleontologist, got interested in what I was doing, and we became close friends. He was an Englishman who had run away from England. It's a long story. His father was a rebel colonel who defied the British government because he believed in the Balfour Declaration and defended it in Parliament. He pointed out that the British government hadn't honored its commitments in the Declaration. Then he came to this country and South America and collected money to hire ships to bring Jews illegally into Palestine. Bryan, his son, was also a very interesting man. There was a whole group of people I met at the University of Chicago. One of them was Dwight Davis, who was the invertebrate anatomist at the Field Museum and curator of comparative anatomy. Everybody hears about "The Panda's Thumb," by [Stephen Jay] Gould. That's Dwight Davis's work on the panda he cites. When I asked Gould about it, he said that he had heard so much about the monograph since it came out that "by now I believe it's my monograph on the giant panda." [Laughter] Dwight Davis was a fantastic, sarcastic character. He was the son of a clergyman, and he was famous for sitting down at the piano and starting to play a hymn and then imperceptibly going

over into jazz. [Laughter] And Dwight wrote this enormous tome—the one Gould cites—on the anatomy of the giant panda, because, you see, the local zoo had a policy that if any animal got injured, they never kept it; they put it away. The bodies went to the Field Museum, and they preserved whatever they wanted. So Dwight had quite a collection of pandas. He finally showed that in terms of comparative anatomy it was unquestionably related to the bears and had to come from bear ancestry.

ASPATURIAN: Was this kind of work also rather unusual in those days?

LOWENSTAM: Yes. But the Field Museum had lots of people like that, so I was able to associate a lot with people who were thinking along my lines. There was Rainer Zangerl, who came from Switzerland. He and Ann, his wife, were also friends of ours. At first they tried to speak German with us, in what's called *Schweizerdeutsch*. We couldn't communicate, so English became the lingua franca. Rainer did a fantastic paleoecological study. He took a black shale from the coal deposits and excavated it layer by layer and located all the fossils through the graveyard, how they were there. Then he described the whole association and reconstructed this and the life history and the current history of these organisms.

ASPATURIAN: Did your association with these people have any major impact on your own thinking? The direction your research took, or the way you looked at some of the research?

LOWENSTAM: It probably stimulated me, but I did my work before they did. But they thought along the same general lines. The head of this whole group was Karl Schmidt, who was the chief curator of zoology at the Field Museum, third or second generation German; his wife, too, spoke German and English fluently, interchangeably. He came from Wisconsin, from a German background—he was very anti-Nazi. They took us really under their wings when we came there. Karl Schmidt and Bryan Patterson edited the little book I wrote in 1948 on the Silurian fauna for the Illinois State Museum. Bryan didn't have much hair. It was generally said he lost most of his hair pulling it out reading my Milwaukee German-English and trying to make pure English sentences out of it. [Laughter]

Schmidt was really amazing. I'll tell you just one anecdote. We would get together every

week at the Field Museum, where we had a luncheon discussion club. After we got through, Davis would sit down at the typewriter, and Schmidt would take Sven Ekman's book on animal biogeography, a book that was written in German. And just as he read, he translated perfectly. He never had to revise anything he said, and Dwight just typed it. And that's how the translation of Sven Ekman's book came out. So that was a fantastic group. They kept track of me when I graduated from Chicago and went to the museum. I would come back every so often. Then the war broke out.

ASPATURIAN: Was this in '41?

LOWENSTAM: Yes. Oh, I should have started a little earlier. We lived in Springfield. I got $100 a month salary, which was a lot of money.

ASPATURIAN: Was it really, in those days?

LOWENSTAM: You couldn't live and you couldn't die on it. Springfield was a very expensive town, because of the politicians—it's the capital of Illinois. When Croneis, the professor, told me, "Your salary isn't very high but this is a small town," he had never been there. As we found out, we couldn't stay there, because the rent was about $70 or $60. So we moved out to Rochester, Illinois, a town of 300 people, about fifteen miles outside Chicago. That's when the war broke out. So a policeman came out; he wanted to know if I had a camera. Yes. He took the camera, gave me a receipt. I got a statement from some higher office that I was not allowed to travel past a certain distance from Rochester because I was an enemy alien.

Then started the school of adult education. One of my pupils was the chief justice of the Illinois Supreme Court. We got friendly in private. I said to him one day, "I wish I could go back to Chicago to check something." He said, "Why can't you?" He had no idea about those things, so I told him. He just stood there with an open mouth. He said, "I can release you. Any time you want to go, you just call my office; I'll tell my secretary. She'll write out the form; I'll sign it, and you can go."

"But I need my camera to take pictures," I said. So he investigated. And you should have seen that policeman come back one day with a long face. He had already taken care of my

camera. It was a Zeiss Ikon Tessar, which was a very exceptional camera—it was 9 x 12 centimeters, postcard size, with a double bellow, and the optics were out of this world.

How I bought it in Munich is an interesting story. I asked Dacqué one day what kind of camera I should get. He said, "Well, if you want to take pictures of your girlfriend, you go and buy a Leica. But if you want to do scientific work, get the Zeiss Tessar. But you might have trouble getting one, because it's not being made anymore." So when I went home that evening to the apartment where I lived, I told this story to the one other person living there, an engineering student. He said, "I have one. I'd like to get rid of it. I want a Leica." [Laughter] So that's how I got the camera. I took fantastic pictures with it.

One day when I was in the Chicago area collecting fossils, I had to go to a place where there was a drainage canal. There was a railroad bridge with tracks running over it, and it said everywhere, "No trespassing." Years before, I had collected there, so when I came back I left my state car on top of the road and I walked down the slope toward the area. And I saw two men up on a bridge. Suddenly they disappeared, and the next thing I knew they were standing in front of me, two old people, with shaking hands, holding a gun: "Hands up!"

ASPATURIAN: You had been arrested as a spy?

LOWENSTAM: Yes. [Laughter] They took me to Lemont, which was a big joke. In the past, when I had collected fossils there, the only place I could stay was a little place owned by a man who was originally from Germany and had a whole box full of German military honors that he had gotten from people who came and couldn't pay for a drink. They would give them to him in exchange for a drink. That was the kind of town I was arrested in. [Laughter]

They took me to something like the FBI—to a basement, where a guy was sitting around. He started to interrogate me. It was perfect, with my German accent. Within five minutes, I had them on the phone calling the chief justice and got the confirmation that he had given me permission to be in this place. So they said, "Next time, don't cross there without getting permission from the railroad company," or the Chicago train system, or whatever it was. They left. I went out. Those two fellows who arrested me were deputy sheriffs who thought those FBI people were stark crazy to let this spy go. But the next morning I found out the reason why all that had happened. The *Tribune* had a big headline—they caught a German spy near Chicago

who had come by U-boat to the Gulf Coast. The local police knew he had been let loose somewhere and was supposed to sabotage strategic places like railroad lines. So they had just caught him, during the night of the day I had been captured. [Laughter]

ASPATURIAN: When was your enemy alien status lifted?

LOWENSTAM: During the war. I was one of the few for whom it was lifted. It was because the Arctic and Desert Division was anxious to have me go to Egypt and supervise the digging of waterholes by Bedouins at El Alamein, and I spoke Bedouin Arabic.

ASPATURIAN: Was this in '43, when the Allies invaded French North Africa?

LOWENSTAM: Yes. I got my citizenship papers the day the Battle of El Alamein was won. [Laughter] The army came looking for me, thanks to Bryan Patterson. He had connections with some people in Washington.

I went to Washington, and they were punch happy I was there. I fit the description of somebody they needed to work with the British just like that. But as I walked out of our meeting, one colonel looked at me and said, "You are a citizen, aren't you?" I said, "No. How can I be? I have only the first papers, because everything has been stopped due to the outbreak of the war." He said, "Oh, my God. You have to be an officer. You have to be a captain at El Alamein, and in order to be an officer, you have to be an American citizen." So they rushed the papers through. [Laughter] But I never went to Egypt, because the battle was already over. Then I got this statement from the Department of Defense, thanking me for my willingness and saying they didn't need me anymore.

What happened next along that line was that I got very unhappy about not being in the armed forces fighting the Nazis. I went to my draft board and said, "What's the matter with you?" They said, "Do you want to volunteer?" Their faces brightened up. "We are short of people. We'll have a meeting in a month from now. You come and we will induct you then." When I went to the meeting, there was a man sitting there I didn't know. I presented my case, and he got up and said, "I'm sorry, I am from Washington. I have orders not to let you enlist in the army. We have enough GIs; we don't need you for that. We want you to go to Urbana,

Illinois, and help develop coal and oil resources. The U-boats are shooting up the tankers going from Texas to the East Coast, and we are short of fuel." That's where I went.

ASPATURIAN: When the army asked you to go with this group, was this part of a military operation, or part of the Illinois Geological Survey?

LOWENSTAM: It was part of the Illinois Geological Survey, but under the auspices of the Department of the Interior, which ran the Geological Survey in Washington. The Illinois Survey was the second largest in the country.

ASPATURIAN: So is that basically what you did during the rest of the war—work on oil and coal resource development?

LOWENSTAM: Not only that. One day, while I was parked in the coal division, a couple of geologists from Shell Oil Company came in with a piece of diamond drill core. They said, "Take a look at that. What do you think that is?" I said, "Silurian fossils, like in the Chicago area." They said, "How do you know that?" I said, "Well, you want to see some?" And I pulled out some drawers. We looked at each other. They had consulted somebody else in Indiana who was an expert on a certain group and he had said it was Devonian—younger than the Silurian period. I said, "What's so particular about it?" They said, "Well, you know, we have drilled a hole, we struck oil. But the Silurian layers are usually at 200 feet. We found these at 600 feet." And I said, "This is very funny. This is coral. Where did you find it?" They showed me the location, and I said, "But that is right where Schuchert said that there was land and no sea."

But nobody really knew what was down there, because before the war, there were very few oil wells going down to these depths. Nobody had ever heard of any Silurian coral reefs as an oil reservoir. When I said they were reefs, I was consulted by the oil companies and wildcatters. As a result of that, I got a three-dimensional picture of this reef, and I was asked to write a chapter for the American Petroleum Geologists Association. They published two volumes on typical American oil fields, and a third volume called "Atypical American Oil Fields." They asked me to contribute to that volume. What I found turned out to be a perfect, atoll-shaped reef.

ASPATURIAN: So there was an atoll reef underneath?

LOWENSTAM: Oh, it was beautiful. It was horseshoe-shaped, which meant that I could even tell from which direction the waves and winds came 400 million years ago, because the head of the horseshoe got its shape from facing into the prevailing waves and winds. Everybody got excited when I published this paper. The oil geologists got all excited, because they were wondering if there would be more oil-bearing structures.

ASPATURIAN: Were coral reefs associated with oil deposits?

LOWENSTAM: I think I was the first to show that they could be, by showing that this unique thing was an oil reservoir. I was never interested in oil, but I was interested in the paleoecology: How far would those reefs go? What controlled their distribution? How big was the archipelago? It turned out, ultimately, that the archipelago went from the edge of the Ozark Mountains to Northwest Greenland and was more magnificent than the Great Barrier Reef today.

After the war ended, the oil companies came and asked me if I would be willing to join them and explore for reef reservoirs. I said I wouldn't do it. I wanted to find out if there were any other reefs, but I wanted to do it as part of the Survey. But when I asked the Survey head—the chief—he said, "No, you're in the coal division; you stay in the coal division." Then a colleague of mine, Carl Bays, who was an old oil hand, said, "Ask the oil companies to give you a letter. Have it in writing." So I got letters from three companies. Carl said, "Now take them down to the chief and show them to him." I went down, the chief looked at me, and said, "Well, if they can do that, we can do it, too." [Laughter] That's how I got permission to investigate the distribution of these reefs by studying all the rock cores first and getting a report together on the distribution and the likely places where there was oil.

One day I came to my office where I always had all my drawers locked, and they were open. Carl happened to come in, and I said to him, "I don't understand something. My locks are broken." He looked at me and said, "You're a stupid dummy." I said, "What's the matter?" He said, "The oil companies know you work on these, and they've sent some spy to find out what's in them." But I hadn't put the possible oil areas on the maps yet, so I decided to find out

who are these companies. I put in some maps with fake locations, and then I watched where the oil companies were drilling. [Laughter] I found out two of the major companies were stealing my oil maps.

I finally published my study, because I wanted the general public to have access to it. That's what I liked about the Survey; everybody had access.

Begin Tape 5, Side 2

LOWENSTAM: In '43, I tried to join the service one more time. The navy came to the Survey, among other places, looking for people who would volunteer to join the navy as a lieutenant, or something, to do hydrographic map work—marine circulation and underwater topography for submarines. Bill Easton, a former schoolmate of mine from Chicago who was at the Survey in a high post, and I were both ready to go, but my boss in the coal division told me, "Go and listen to them. I don't want you to go, but if you try to, I have to give you permission before you can leave here." So we went over. Bill Easton was in the oil and gas division, so he had no restrictions. He signed right up. It sounded interesting to me, but when I went back, my boss said no, he needed me here and would fight it all the way. So I didn't go. The big joke was that Bill Easton ended up not in Washington in the hydrographic office but as an officer on a destroyer in the South Pacific.

ASPATURIAN: Which is not what he had planned. You, on the other hand, wanted to go fight.

LOWENSTAM: I did a few things along those lines. I interpreted aerial photographs for the air force. They had pictures showing that the Germans were suddenly modifying their coking ovens in the Ruhr district, where they processed coal, and they wanted to know why. I realized that one may extract high-octane fuel for aircraft from the coking coals. The Germans were resorting to this as a temporary measure, because they were so low on aviation fuel. When I explained this, the air force bombed the plants, put them out of business. So I got my revenge that way.

ASPATURIAN: How long after the war did you stay with the Survey?

LOWENSTAM: Two years, until early '48.

I want to tell you one more interesting thing about the Rochester days. That was a little town—300 people—and we were told that nobody who lived there for less than three generations was accepted by the local people. But they accepted us just like that. The mayor would come over and sit on a bench with me and talk. We had a house that originally went back to the days of Abraham Lincoln. The original owner had been a client of Lincoln, and his son—who had a different house by then—showed me pictures where he sat on Lincoln's knees while Lincoln and his father were conducting their business affairs. He was about eight at that time. Also, in Rochester, I met a couple, the Verals. Victor Veral was the son of an Iowa farmer; both parents were British, highly educated. He was an engineer. We became very close friends, and learned a lot, through his wife, about modern music, and from him a lot of British literature, which we didn't know. So that was the little village.

The funniest thing that happened there was one day the mayor came and sat on the bench with me, looking very uncomfortable. I said, "What's wrong?" He said, "Well, the people here have noticed that on Saturday you don't go to Springfield to the temple. Now, we are not prejudiced. We believe that the Catholics should go to the Catholic church, and the Protestants to the Protestant church, and the Jews to the temple. And everybody does. But you don't." He said, "Do me the favor, go," and laughed. So from then on I went every Saturday at the right time to Springfield, where I worked at the museum until the time was right to come back, and then I could see all the spyglasses going, everybody checking. [Laughter] They were satisfied that I came back at the right time. I must say that I became more integrated and learned more about American people and the life of America living in Rochester than I have anywhere else. They were fantastic people. Also, my former wife was a physician, and she was a country doctor. So I met all the farmers, a complete cross section, and we were on good terms.

ASPATURIAN: So she practiced.

LOWENSTAM: Yes. I drove her the first time, when I had barely learned to drive, because a man had had a heart attack. I went zig-zag across the road, but I made it. [Laughter] Then, as we left the place—a big farmhouse in a hilly region—I looked and the car was gone. I hadn't put on the brakes, so it rolled. But it got hung up just before it reached the street, which was fortunate, because that road went to Taylorville. Rochester County was dry, and Taylorville was wet, so all

the drunks were coming back from that direction. I was very lucky not to lose the car—I had borrowed money from my friend Jim Meyers to make the down payment.

ASPATURIAN: How about the heart attack victim? Did he live?

LOWENSTAM: Oh yes. In fact, that was the big start of my wife's career. When people heard that she wanted to be a country doctor where no one knew her, they said they didn't think there was a chance. But after word got out about this patient, it made people in the area take another look, and she got lots of patients.

ASPATURIAN: How did you end up back at the University of Chicago, working for [Harold] Urey?

LOWENSTAM: When I was in Urbana, Dr. [Everett C.] Olson, a former vertebrate paleontologist in Chicago and dean of physical sciences at the University of Chicago at the time, came over one day and asked me how I would handle a course in invertebrate paleontology. I gave him an outline, and I didn't hear anything. I found out later he tried to get me on the staff and was turned down because Tom Barth, a Norwegian who was head of the department in the early forties, before he went back to Norway, had been asked by the administration, What was the future of geology? And he had said, "Geochemistry." So Chicago decided they were not going to hire a paleontologist or anything like that, and they turned down Olson.

But then Allen Riley, who was a former classmate of mine and was by that time an assistant professor in Chicago—he ended up as director of Chevron Research—had lunch one day with Urey. And Urey was very upset. He had calculated that there should be temperature-dependent thermodynamic effects on the oxygen isotopes. He had given a lecture on the subject, not only about oxygen but about a number of other stable isotopes, to the Royal Society of London. And [Paul] Niggli, the famous mineralogist from Switzerland, got all excited and got up and said, "Professor Urey, do you realize that if this is correct, you have a quantitative thermometer tool in your hand? You will be able to measure the temperature at which magmas crystalized at depth, and so on." Urey came back to Chicago and started to get a group together. He wanted to run paleotemperatures by means of fossils.

ASPATURIAN: Was there anyone in his group who had that expertise?

LOWENSTAM: No. He went to a colleague who was there—a very good paleontologist who had given him some fossils, and the results they got didn't make any sense. So Urey wanted to consult with somebody who knew more about it. When Allen heard this, he told Urey to ask me to come over and talk to him. So Urey invited me one day to visit his lab at Chicago. He showed me everything.

ASPATURIAN: Did you know anything about his work at that time?

LOWENSTAM: No.

ASPATURIAN: Did you know who he was?

LOWENSTAM: Yes. By that time, he had the Nobel Prize for having discovered heavy water—deuterium. So that I knew. But I didn't know him, nor had I given his problem any thought. He said to me, "Now look, do you think that in Mississippian times"—that was about 300 million years ago, roughly—"the water temperatures of the oceans could have been something like 60º or 70º Centigrade?" I looked at him and I said, "What do you think would happen to the animals?" He laughed and said, "Probably die." I said, "Yes, they would." He said, "How do you explain this? Here's a perfect fossil of a living animal from that period." He brought it out, and it was perfect physically. I said, "Do you have a thin section of that?" You know, you make a very thin cut and you examine it under a microscope to see what the crystallographic orientation is and what the texture of the minerals are. When I asked him this, he said, "Yes. Why? It's perfect calcite material." I said, "I know, but may I see it?" I took one look, and I said, "Look, this is an echinoderm. Echinoderms have very unique microarchitecture when they're alive. And then they're well preserved, the skeletons. This one is recrystallized."

I explained to him that it was originally like what he had. I said, "You probably got the temperature of the water right, but what happened was that after they died they would recrystallize and the perfect structure would be preserved."

"Oh, my god!" he said. "Yes. That would make sense."

We talked about other things, and finally I said I had to go back. He said, "OK, when do you join me?" I looked at him, and I said, "What do you mean?" He said, "I need you!" [Laughter] He was one of those people who, if he wanted something, it was "I need you now." I told him I was at the Survey and I couldn't just up and leave.

"Never mind." So he went to [Walter] Newhouse, the head of the geology department at Chicago, and he said, "Can you do something about it?" Newhouse was rather reluctant. It was an ironic situation. When I came to the Survey from the State Museum, the chief of the Survey was the head of the museum board, and he was very hesitant to hire me, "except that the War Commission ordered me. Otherwise, I couldn't hire you, because it would be unethical to take you away from the museum." Newhouse at Chicago was the head of the board of the Survey. [Laughter] As I say, he was at first reluctant, but finally the appointment went through and they asked me what I wanted to be. They said they could hire me as a research associate in biogeochemistry. That was Allen Riley's invention.

ASPATURIAN: What was that supposed to mean you were doing?

LOWENSTAM: [Laughter] Working with Urey on his oxygen isotopes. But there was chemistry in the word, and hence it would be acceptable to the administration. I said yes, I would be willing to be a research associate in biogeochemistry, provided I would be in the geology department, and glad to collaborate with Professor Urey. But I told them I also had my interest in the Silurian and wanted to continue my work on the many unanswered questions about the reef development. Urey and Newhouse agreed to that, so I became a research associate in '48, less than two months after I had seen Urey for the first time. [Laughter]

ASPATURIAN: Was the kind of interdisciplinary title they gave you common in those days?

LOWENSTAM: Not at all. As a matter of fact, my old profs from the geology department were making jokes about the bio-geo-so-and-so-forth, attaching new syllables every day.

ASPATURIAN: Was geochemistry a separate department at that time, or was it a subdivision of

geology?

LOWENSTAM: Chicago didn't have a geochemistry department at that time. But in Europe, Finland had pioneered it, among others. There was [Kalervo] Rankama, who was a visiting professor in Chicago as a geochemist, and there were young assistant professors, a couple from Norway, who were geochemists. That was just coming in. Rankama called me in one day. He was writing a book on geochemistry. He threw a bunch of file cards at me and said, "Take a look at that. They deal with trace-element chemistry in shells and other marine organisms. You might be interested in it." I got all excited about it. Urey had opened a new avenue to quantitatively determining paleotemperatures, and now here were possibilities for using trace-element chemistry to tell something about genetic and ecological expression.

Geology at Chicago really opened up after the war, and it was very exciting. I always said later that if they had asked me when I graduated—which they never would have done—to join the staff at Chicago, I would have said no. But when I came back, the place was jumping. It was the most beautiful situation. In geology we organized a series of seminars—I was maybe a little instrumental in it—between the Institute for Nuclear Studies, the physics department, and biology and geology. Enrico Fermi came, and so did Urey, Bill [Willard] Libby, and Leo Szilard. You name them, they were all there. It was the most exciting period in my life. There were new things everywhere. Libby was working on the radiocarbon isotopes at that time. He shared an office with Urey, so I heard every day about that, too. We drank with them the famous old wines that were used to calibrate the age determination column. If you knew a certain wine was 200 years old, you knew what the radiocarbon decay rate was. Jim Arnold would always call up and say, "Today we open a new bottle of aged wine." [Laughter] Really aged. They invested several hundred dollars for this.

ASPATURIAN: What was Urey like to work with as a person?

LOWENSTAM: I found him very exciting and stimulating, of course. He changed my whole outlook and added an enormous new facet to what I was interested in before, which was complementary but quantitative. He got interested, also, in learning something about fossils, and at the same time he offered fatherly advice to me. Like saying, "You never write a paper that

says you have done this and that and that, and it didn't work. You don't talk about it. Finally something works, and you say, 'Here's the proposition. Here's what I did. Here's what came out.'" [Laughter]

He came along with me on my first big field trip to Tennessee, and clear down to Florida and Alabama, and the East Coast. We came to Philadelphia, to that area, collecting fossils. I'll never forget him at the first outcrop. He was like a kid with a little new toy. "Come over here, come over here! Look at this fossil! What is it? What does it mean?" It was such a pleasure to see an older man being so enthusiastic. I had to restrain him at the beginning, because otherwise I couldn't get much done. He just was all over the place. When we came to the Pee Dee River in South Carolina, where we saw an outcrop of some extinct cuttlefish concentrations, he was ready to grab. I said, "No, Harold. We first take a picture. We number and label each specimen. Because how else do we tell how that deposit's been laid down over several million years?" By that time I had him in the harness. He helped me, and we worked as equals doing that. We come back to Chicago, run the oxygen isotopes, and find out they are all the same temperature. They were all from the same geologic period. But it could have been very exciting if it had been different. So I learned a lot from him.

On that trip, I finally said to him, "Tell me one thing. Why did you not follow Niggli and go into petrology, determine temperatures of minerals? Why fossils?" He had insisted that the first thing we'd study was the end of the Cretaceous—the time of the end of the dinosaurs—and I was curious about that, too. He said, "I'll tell you a little secret. I used to teach biology when I was a young man in Montana. I read all about dinosaurs, and got all excited about it. I always wanted to know why they died out. Maybe there was a climatic change—a chilling or something." So that decided him. While he was wrong about why the dinosaurs died out, he published later, in '73, a paper where he said that at each major boundary in the geologic column there was evidence of an asteroid or meteorite [impact] effect ["Cometary Collisions and Geological Periods," *Nature* 242(5392): 32-33 (1973)]. That early, he was thinking along the same lines we are now. He wasn't so far off with the temperature either. The latest date I showed, there was first a low and then a high spike. So he was really in the right ballpark.

ASPATURIAN: As an individual, as a person, what was he like in that respect?

LOWENSTAM: He was ruthless, in a way. When he wanted something, he would go over the bodies. One day I came to his house for dinner. I came in with Urey, and Johnny, his little boy, went over to his father, eyes all big, and said, "Dad, you're a hero." Urey looked at him, and Johnny showed him a cartoon—it was apparently privately published. It was called, "Schmurey the Schmu." It showed the captain standing in front of a ship and saying to a guy, "Dive, and get me a sample," and so on. Urey got red in the face and said, "I wish I knew who that guy is. I would…" Johnny looked at his father, and he couldn't understand. Here's his hero, suddenly so upset.

ASPATURIAN: Did Urey drive his people very hard?

LOWENSTAM: Yes. For instance, I lived in Homewood, which was about twenty miles south of Chicago. I didn't want to live in the city. That's where we got together again with Dwight Davis, Bryan Patterson, and Karl Schmidt. We lived in the same neighborhood, as a matter of fact. Urey couldn't stand it that I couldn't be reached within two minutes, that it would take at least ten minutes to get me by phone, or something like that, and that I was not there all the time. He would tell people that we were the only people he knew who took the atomic bomb seriously and had moved out of Chicago to the toolies. [Laughter] He wanted everybody right there and all the time. I finally told him, "I do my work, and I enjoy it; and it's great. But I have other problems to work on, too." It always went in one ear and out the other. But I enjoyed very much working with him.

I continued my work on the Silurian reefs at the same time. I edited a special issue of the *Journal of Geology*—a colleague of mine, Francis Pettijohn, had asked me—on fossil reefs. The issue included two papers of mine and really gave a picture of the reefs. Urey gave me the chance. I had wanted to start much closer to the present, like the Ice Age or the Tertiary, because I felt we were on safer ground about whether we were in the right ballpark or not. Urey insisted on 100 million—or at least 65 million—years back. So I said we ought to at least determine what is equilibrium and what is disequilibrium, so that we don't make mistakes. It took a long time, but he finally gave me permission to go to Bermuda and collect modern communities of marine organisms, where I knew the temperature relationships. Then Sam Epstein and I published a study on that ["Temperature/Shell-Growth Relations of Recent and Interglacial

Pleistocene Shoal-Water Biota from Bermuda," *J. Geol.* 61(5); 424-438 (1953)]. In 1950 I also went to Palau, in the South Pacific, for the first time. I wanted to have one area where the water temperature is uniform the year round and not in a tank, where we never know what variables we overlook in nature. Also, the water chemistry there is very uniform, since it is in the open oceanic reservoir, with no salinity changes. So I went to the South Pacific, and thanks to some friends I was directed to Professor Hatai, the former Japanese director of Palau Marine Biological Station, the first tropical marine station. He was a wonderful man. He sat down and copied for me all the water temperatures they had measured over a period of fifty years—I still have that fine handwritten paper—showing that the temperature was 28° plus/minus 10º centigrade, maximum deviation over a period of fifty years. So that was a thermostatic situation where whatever didn't fit would clearly show up in the isotopic composition. The water chemistry was that of the oceanic reservoirs. There were no salinity variations to affect the isotopic composition, and we were able to eliminate all kinds of variables. Everything was fine. And I'm still working on it today. That's where I found later the chitons with their iron teeth.

HEINZ LOWENSTAM

SESSION 6

July 19, 1988

Begin Tape 6, Side 1

ASPATURIAN: When you were at Chicago, did you teach?

LOWENSTAM: Yes, but only later. I came to Chicago with the understanding that I wouldn't have to teach.

ASPATURIAN: You didn't want to? How come?

LOWENSTAM: I had a phenomenal teacher at the University of Chicago when I was a student—J. Harlen Bretz. He was a born teacher. On field trips, for instance, he would get discussions going, and if things got off the track he would get people back on the track so smoothly that hardly anyone noticed. I admired him. So when I graduated, I looked at myself and decided I wouldn't be any good at teaching. I wanted to do research. When I came to Chicago, I insisted that I would only do research, but after about a year Urey started to get after me, "You have to teach. How else are people going to know what you are doing?"

I said, "It doesn't matter; it's not important."

"Look," he said, "I teach introductory undergraduate thermodynamics. That's how I get the good students I want later for PhD work."

I still said no. One day I got a phone call from Vice President Harrison. He said, "Please, teach one course to get Harold off my back." [Laughter] I don't know why Urey was so insistent. I started to teach a course and I taught it for two years. It was very interesting and exciting and the students were very enthusiastic. I would come back to my office at night, if I had to close the trap or put dry ice in the mass spectrometer, and here were my students still discussing things we had talked about earlier in class. So I was quite intrigued.

About 1951, suddenly things changed. I had great difficulty in teaching. I walked down the steps one day at Rosenwald Hall and ran into Tom Barth, and I said, "I don't understand what's going on. Up to now, everything was fine in teaching, and now I simply can't do it as

easily." He looked at me and said, "Aren't you aware of what's going on?" I said, "No." He said, "Before, you had the GIs coming back from World War II. Those were mature people. They knew what they wanted. And now you get the normal run-of-the-mill students you ought to have at that stage." I had never taught before, so what he said made a lot of sense. We finally got things going, but I had to use different techniques.

ASPATURIAN: In 1952, I believe, you came to Caltech. What led you to come here?

LOWENSTAM: Harrison Brown had been after me to come here for about two years. He had accepted a position here. He had been at the Institute for Nuclear Studies at Chicago, and he was hired away by Caltech's geology department around 1950. They were making a lot of changes in geology at that time. There was a rumor—I never checked it—that apparently Linus Pauling had looked around and felt geology was not on par with Caltech's other divisions.

ASPATURIAN: In terms of its geochemists?

LOWENSTAM: No, in general. Harrison said something like that. So he had already gotten Sam Epstein, and Chuck McKinney, who was in Urey's lab, to come here. Clair Patterson was about to come. And he also wanted me. About the same time, I was invited here to give a seminar, but it had no relation to what Harrison was doing. He didn't know anything about it. What I found out later was that the department had written to a colleague of mine in Chicago—Francis Pettijohn—asking him to recommend some young people in paleontology and sedimentology. Pettijohn had asked me whom I would choose, and I had recommended a student of mine, Dick Bader, who'd just graduated and had done the first work on organic content in recent marine sediments. He was an older boy who had been in the war and stayed on afterwards to write a number of manuals for the air force. Pettijohn wrote a letter with my recommendation, and apparently—this is, again, rumor—he pointed out that Bader was one of my students and that I was leading in a new area. So they finally decided, instead of getting a student, to try to get me!

I came here to give the talk. It was very interesting. There was a colleague here—Al [Albert] Engel—who went later to Scripps. You've probably heard of [Fritz] Zwicky. Al Engel was the Zwicky type. If anybody said anything he disagreed with, he would jump in and try to

say, “That’s nonsense!” [Laughter] When I talked about the origins of cherts—flint—based on my studies in Illinois on the Silurian, I said that I was greatly surprised to find out that the textbook ideas were incorrect. It was generally thought that they were not formed by inorganic precipitation from seawater, as the textbooks said, but that they were derived from sponges in the Silurian with siliceous skeletons originally, and when [the sponges] died, that was the source of the silica for the chert. But I said that what really surprised me was that there was a Professor Tarr in Missouri who had published a series of papers in which he showed that younger cherts from the coal age—the Carboniferous—in the Midwest were all formed by inorganic precipitation from seawater. I had just come, the week before, from visiting an outcrop of one of his places. I found that the cherts were full of sponge spicules, and I couldn’t understand why he hadn’t seen them. The only reason I could think of was that he used the microscope with polarized light, so that he only looked at the crystal arrangement and not at what was there. Al Engel got up angrily. He said, “I come from the University of Missouri. Professor Tarr was my professor. How can you state that?” I said, “I happen to have the slides here, showing thin sections of the material.” I showed the most perfect case I had ever seen up to this day, not just spicular aggregates but nodules full of spicules. After I said that, Al Engel sat down, didn’t say a word. I didn’t know his reputation. I heard some snickering around me but I didn’t know what was happening. Apparently it was one of the rare occasions when somebody had been able to silence him. [Laughter]

I later had the same experience at La Jolla, when he was there, over the issue of magnetite. He had a loud objection—his Professor So-and-So had said that it couldn’t be. I said, “Wait a minute, Al. I’ll show you a few slides and explain what is going on.” [Laughter] We were good friends.

ASPATURIAN: So Caltech offered you the job.

LOWENSTAM: Right. But the curious thing was, I didn’t want to come. Chicago was so exciting in those days. I consulted with a whole series of acquaintances and friends who I thought would say I should stay. I remember I asked Leo Szilard, and he just looked at me funny and said, “What are you waiting for? Just go.” And Karl Schmidt at the Field Museum said, “Well, there’s a new museum opening in LA. We need an equivalent of the Field Museum on the West

Coast. They just got a French director who is independently wealthy."

ASPATURIAN: Was this the Page Museum?

LOWENSTAM: No, it was the LA County Museum, downtown, across from USC [University of Southern California], before the Page Museum was built.

Karl said, "The new director needs help, and you can help him. So you must go." [Laughter] That was his argument. Urey was one person I didn't ask; I knew he wouldn't say I should go. He wanted me to stay at the time, that's why I didn't ask him. But I went through a whole series of people, and they all said I should go, every one of them. The big joke was that within five years after I came they were all out here. Urey went to La Jolla—he was Professor at Large, actually, at the University of California. Then Bill Libby, who got the Nobel Prize for discovering radiocarbon dating, went to UCLA. Leo Szilard went to the Salk Institute at La Jolla. And so on.

When I came here, Caltech already had what was called the Chicago Mafia, because of all the people Harrison had hired. Sam Epstein, with whom I had worked with Urey and published with, had come the year before. I was supposed to have come earlier, but I had to finish up certain things.

ASPATURIAN: At the time you came, was Caltech sort of staging a raid on geology and geochemistry across the country and bringing young people in?

LOWENSTAM: Yes, I think so. I don't know if it was any of Pauling's doing. It could be. A lot of new young people came in about the same time I did, although not in seismology, because there were old-timers who were still very active: Beno Gutenberg and Hugo Benioff, who was a wonderful fellow; and Charlie Richter—a young man in those days. In those days, the emphasis was still on geology and geochemistry. In paleontology, apparently, there had been a whole series of people here for a very short time—only one at a time—with the exception of George Gaylord Simpson in vertebrate paleontology. Chester Stock, a vertebrate paleontologist, was chairman before Bob [Robert P.] Sharp, and the geology students often wrote their master's theses on vertebrate paleontology. But invertebrate paleontology was apparently not highly

regarded. So a number of very famous people had been here for a very short time and then left because they got other offers. They had more rewarding contacts with outside people. People like Al Engel and others just looked down on paleontology as a service they could use to assign ages to rocks. I wasn't aware of that, because I didn't know much about Caltech.

So I got here, and was for a while alone. I got young colleagues in invertebrate paleontology, but they always left after a short time. Some left because they didn't feel they were very welcome here, in general. I paid no attention to it; I did always what I wanted to do. I remember they asked me what I wanted to be called, so I told them—professor of paleoecology. And George Gaylord Simpson, the famous vertebrate paleontologist, one day asked me, "What's it all about, paleoecology?" I said, "Well, George, when you want to do anything you want without having to justify it, you call it paleoecology." He laughed, and said, "That's pretty clever." [Laughter]

ASPATURIAN: What were your early impressions of the department and some of your colleagues?

LOWENSTAM: Well, it was a very solid department.

ASPATURIAN: Intellectually solid?

LOWENSTAM: Intellectually solid, but basically field-oriented—geologic mapping played a big role in it. And although I appreciated that, I was not involved in any of it. In contrast to when I was at the Survey, I was often an outsider here, in a way.

ASPATURIAN: Did that bother you?

LOWENSTAM: At times, yes. I mean, my colleagues and I were personally on very good terms, but I felt very isolated. But it never occurred to me to leave—I had quite a few offers to go to other places. In a way, I enjoyed the fact that I could do what I wanted to do. I felt that that was the most important thing. I often did exploratory studies that were considered rather outside the area of geology and more relevant to oceanography or biology. For instance, we had the first

open, high-pressure aquarium, so that I could see what hydrostatic pressure did to marine life. I wanted to see if I could find a quantitative depth factor in paleontology and sedimentation, because up to this day there's still only talk about what depths extinct marine organisms existed at. Even today, I don't have much confidence in estimates that arbitrarily assign a deep-water origin to certain fossils. So I thought maybe I could quantify that phenomenon by a hydrostatic pressure experiment, because pressure would not be a variable over evolutionary time; it would be constant.

I actually started this work out on a sabbatical, around 1962. At that time, Caltech didn't have a regular sabbatical in any division. But Bob Sharp, who was geology division chairman then, thought it was a good idea to see if we could use my case to push through a formal sabbatical policy for all of Caltech. [Laughter] I was the guinea pig.

I went first to Sweden. In Stockholm, I wanted to do some field work on the island of Gotland in the Baltic Sea. I also went to the Natural History Museum in Copenhagen, because they had been involved in the first detailed expedition on deep-sea life and had brought back phenomenal things. One of the things they had discovered in the deep sea was an animal that was supposed to have been extinct for 350 million years. So when I went to Copenhagen, I asked around, "Are there any organisms that are found today in environments ranging from fairly shallow water to really deep sea depths?" They had all the deep-trench life records. I got a number of groups together, and we started to look into the trace-element chemistry.

ASPATURIAN: And to correlate that with depth?

LOWENSTAM: With depth, right. I discovered that there was a pressure effect from 5,000 meters down. But it varied according to the species, so it wouldn't be part of the fossil record. So, I found it but I couldn't use it. But then about that time we had already started on the high-pressure experiments, thanks to Jim [James A.] Westphal. He was phenomenal. He came along to Palau, to the Caroline Islands. Of course, he liked diving and underwater photography. So I had him along, because I didn't want to be bothered with that. I wanted to look at things.

One day I said to Jim, "Do you think you could design a high-pressure apparatus that operates in an open system?" I didn't believe previous results on high-pressure effects, which showed that various organisms were dying after the pressure got up to certain levels. All these

experiments were made in closed systems, and what would happen after a while is that the organisms, by respiration, would reduce the oxygen content of the water. I suspected that the organisms weren't dying because of higher pressures but because they had used up the available oxygen. When I asked Jim about an apparatus, he said, "I don't know. I'll think about it." That was in the afternoon. At night, about two o'clock, I was rudely awakened by Jim. "I have it." He had it all worked out. Within six months we were operational. Then the Office of Naval Research got quite interested.

ASPATURIAN: They funded your research on this?

LOWENSTAM: Yes. They had asked me what I would do if I had the money, and I said [I would do] this kind of work. I was lucky I got Jim. It had its funny side, too. About that time, I went on a trip to Europe, on the leg of a research cruise from Nice to Brunei, going through the Mediterranean. I was interested in certain organisms in the Mediterranean at that time, so that was why I was there. On the way back, I visited Jacques Cousteau, since I knew him. I had met him before at an international oceanographic conference in Bermuda, and at that time I had tried to get his underwater vehicle to get a certain sample. While I was visiting, Cousteau asked me what I would do next. I told him about the high-pressure work. But Jim hadn't finished his design yet, so we weren't sure it could be done. I didn't tell him that. He said, "Oh, we are thinking along the same lines. Of course, we will be first." I said, "Is that a bet?" He said, "Yes." So we had a bet on it, and I forgot about it.

Then, about 1970, we had the first Biology International Conference in South Carolina. I gave an introductory talk on hydrostatic pressure effects on marine organisms, and in another talk I talked about our instrumentation and the results we had gotten, which were very interesting. I could show that for certain organisms, the maximum depth at which they are located is determined by the maximum pressure they can tolerate. We had tested that here in our high-pressure aquarium and demonstrated that it was so. After the talk, a man came over and asked if I would be willing to send him the circuitry of our instrument. And I said, "Of course. By the way, who are you? Where are you from?" He said, "I am from France. I work with Jacques Cousteau." I said, "Oh. I'll tell you something. You go back and tell Jacques, when he sends me what he promised me first." [Laughter]

ASPATURIAN: Did you hear from him?

LOWENSTAM: Oh, yes.

ASPATURIAN: You mentioned that you were kind of out of the mainstream. Do you feel that your work was properly appreciated, especially in the early period? Did your colleagues seem to understand and appreciate what you were doing?

LOWENSTAM: Not quite. Except Bob Sharp, who had confidence always. He supported me.

ASPATURIAN: He was the [division] chairman at the time you were hired.

LOWENSTAM: Yes, he was the chairman. He never questioned what I wanted to do, and was always pleased about the results I'd gotten. He was really fantastic. I think that the division did so well under him because of his attitude. Also, he was a very clever man. When he was chairman, you got the impression that he was only the errand boy for the faculty for what ought to be done. But if you carefully watched in some of the staff meetings, particularly at his house, he had certain ideas and he knew how to get them through without anyone thinking that he was trying to pressure the faculty. He had broad visions of what should be done, and I thought it was fantastic the way he did it. He was responsible for this division becoming what it did.

ASPATURIAN: Who were the major people on the geology faculty in the first decade you were here?

LOWENSTAM: Al Engel was one. He was a very good man in petrology. Dick Jahns became dean later at Stanford. He was a great field man and a fantastic teacher, too. And, of course, the Seismo Lab was major.

ASPATURIAN: Did you know [Charles] Richter?

LOWENSTAM: I knew Richter only casually. I knew Beno Gutenberg; we were very close friends. And also Hugo Benioff.

ASPATURIAN: Can you describe Gutenberg?

LOWENSTAM: Gutenberg was so broad in his approach to lots of questions, and he was such a personable individual. I never heard him use a sharp word. He was hard of hearing. He had a big hearing aid with a regulator. I remember we would be sitting together somewhere. When I saw that angelic smile on his face, I knew he had turned off the hearing aid, so if I wanted something, I would point to his hearing aid. [Laughter] You could talk to him about anything—seismology, tectonics, climate—you name it. And he had friends always who were first rate in these things. Hugo Benioff, too, was a great man.

ASPATURIAN: What was his field?

LOWENSTAM: Seismology. But he started out, apparently, as a high-powered instrument maker. All the good seismographs all over the world were his invention and his design. I just remembered a funny story about him. He was in Rome at the international seismologic meeting. He knew that one of the cardinals in the Vatican was a seismologist, but he had never paid much attention to him. The cardinal came to the meeting and asked Hugo if he wanted to see his instrumentation. So Hugo said, "Sure!" They got onto the grounds of the Vatican and they came to a big, high wall. There was an iron ladder, and they went over the ladder and into an enclosure—four sides with these high walls—and in the center, on the open ground, was the seismological station, hidden from everything else. Hugo was very impressed by the cardinal's knowledge, and the cardinal said it was Hugo's instrumentation that made it possible to do his work. As they went back over the wall to the general grounds, Hugo turned to him and said, "Tell me, how do you reconcile being a cardinal and a seismologist?"

"That's very simple. When I'm on this side of the wall, I'm a cardinal. When I'm on the other side of the wall, I'm a seismologist." [Laughter]

Hugo was a very broad scientist. He was the one who showed that there is the circum-Pacific Rim movement of the continents counterclockwise. He was the first to measure the strain

at the fault lines along the San Andreas Fault. He got huge quartz crystals and put them across the fault and watched the strain effect on it. He was really phenomenal.

Hugo Benioff was famous for another thing. He developed an electronically controlled concert piano, entirely run by electronics. He took decades to complete it, because a lot of theory wasn't known; that came much later. And finally he had it. We were often invited to his house to hear the various stages of development. As you know, the reason a piano doesn't have a sharp tone like a harpsichord is because of the felt pieces at the end of the hammer. Hugo spent a long time working out how he was going to duplicate that, but he finally got it. Very famous pianists came. They would look very dubiously at that instrument—no strings, and everything controlled by electronics—sit down and try a little bit. And then they would suddenly perk up. I remember he couldn't get some of them out of the chair to stop playing. It was so superior that for years I couldn't listen to any other concert piano. The sad thing is that it was financed by Baldwin, the piano manufacturer. Baldwin had supported Hugo all these years to do it. Hugo had really wanted to build an electronic cello, but Baldwin talked him into a piano. So when it was finally completed and there was a concert at Carnegie Hall, people raved about it. It was the one and only concert where this instrument was ever used. Because shortly afterwards, Baldwin's president died. His son took over and got some efficiency experts in, and they said that each electronic piano would cost about $100,000. They suggested he scratch the program, so they mothballed the whole thing. Hugo never got over it. When he retired to Mendocino County, he tried to build an electronic cello, but he died before he could finish it.

ASPATURIAN: Were your major associations in the division with those two people? Or did you associate a lot with the geochemists also?

LOWENSTAM: I also worked with Harrison Brown. We taught one course.

ASPATURIAN: Did you collaborate on projects with him?

LOWENSTAM: Not on projects, because he was off on meteorites at the time. And I tried to stay on this planet. I felt there were still a few problems worth considering here. [Laughter] But we taught geobiology together. That was an interesting course, because the students who attended

were biology students, math students, physics students—they were from all over. Hardly any geologists. Harrison quit teaching the course later.

ASPATURIAN: Were there any geobiologists as such?

LOWENSTAM: No, none.

ASPATURIAN: So the two of you must have pretty much designed and invented the course.

LOWENSTAM: Right, we did. We had lots of fun. We spent a lot of time privately together. We used to go on hikes.

ASPATURIAN: Did you have detractors in the department—people who felt that this kind of approach was useless and not really related to the true path of geology?

LOWENSTAM: Well, they were mostly not against it per se, but they were afraid that it would take over and replace geology. So they became more reactionary, in terms of insisting that there had to be lots of field courses and field-related courses and lots of petrology in the curriculum. They were probably not so wrong.

ASPATURIAN: I was wondering to what extent this mirrored disciplinary debates going on around the country with regard to the direction geology should take.

LOWENSTAM: I think we were way ahead at the time, here at Caltech.

ASPATURIAN: In what ways specifically?

LOWENSTAM: Well, we stressed geochemistry more—in the sense of recognizing it as a link to biology and paleontology. I got some good students in the field.

ASPATURIAN: Whose influence do you feel was primarily responsible for the fact that Caltech

turned out to be a leader in some of these new areas? Who were the moving forces behind it, both inside and outside the division?

LOWENSTAM: Certainly Linus Pauling was supporting it very strongly. There were always very good relations with physics and astrophysics. And astronomy even conceded the solar system to us. [Laughter]

ASPATURIAN: Why did this happen here, in your view? What allowed that to occur?

LOWENSTAM: I think because of Bob Sharp, very definitely. He took a rather balanced approach, in that he tried to maintain field geology, which was important to him, and he was a virtuoso in that respect. People came from all over the country just to go on his field trips.

Begin Tape 6, Side 2

LOWENSTAM: In fact, what he was most famous for was field geology. But he also always looked at what would contribute to get geology more rounded, more dynamic. But sometimes that's difficult.

They finally gave me a chance to get more typical paleontologists here, but also with biological ideas. One was David Raup, who has been in the limelight quite a bit lately because of his work on extinction sequences in the evolution of life—that every 26 million years there has been a mass extinction due to an impact event. He and [J. John] Sepkoski, one of his former students in Chicago, were here. But he left. This is very funny. He didn't want to leave, but his wife was one of the New York society girls and she hated being in the Wild West. I remember she would come to parties at our house and look at people like they were animals in the zoo. And the joke is, they later got divorced. Dave told me once that he was so happy here, but she put so much pressure on him to leave. He got a chance to go to Johns Hopkins.

Another one we hired was Frank Staley, who had just graduated from Columbia. He was very bright, and got influenced a little bit by me and others. In his case, it was money—the oil companies—that lured him away. [Laughter] I'm laughing because one day I was asked by an oil company to join its research staff. I said I wouldn't, and they said, "Do you have anybody else? Let us know." I happened to walk by Frank Staley's office—he had been here barely two

years—and I said, "You ought to have a school friend from Columbia you might want to recommend for this job." I gave him the letter they had written to me. The next thing I knew, he had gone out and taken the job [himself].

ASPATURIAN: Did this come as a shock to you?

LOWENSTAM: Yes. For a while, I had lots of trouble with the oil companies, because they were always hiring my students for their research labs.

ASPATURIAN: Right after they graduated?

LOWENSTAM: Yes. One time, I had two students graduating—Mike [Ronald Michael] Lloyd and Bob [Robert J.] Stanton. Mike Lloyd had been supported while he was a student here by Shell Research. He was married and already had a family. I knew him when he was a student at the University of Illinois, and I supervised his master's thesis, among other things, there; then he came here for his PhD. One day the head of the Shell group came here and said, "Well, I just hired two of your boys." I said, "What do you mean two? I know about Mike Lloyd." He said, "I got the other one, too." And the other guy—Stanton—was a straight paleontologist. I was rather disconcerted about the whole situation for a while, because they were going to work right and left for Shell, Texaco, you name them. But within five or ten years, at the most, they were all back in the academic field. They just took a few years off, and then went back.

ASPATURIAN: Was there much independent consulting among geologists and outside companies?

LOWENSTAM: There was lots of it, especially in oil and ore deposits—iron, lead, zinc.

ASPATURIAN: Was this a bone of contention in the division, or was it pretty much taken for granted?

LOWENSTAM: We had one staff member who was a main consultant to US Steel. Al Engel

forced him out, more or less, because he thought his students were just doing flunky work for US Steel. I can't think of his name, but he was the world expert on fluoride deposits, which was very important to me, because when I discovered that organisms do the same—excrete fluoride—I went and asked him if he had ever seen any fluoride crystals in the shapes that organisms form. He said that once he had seen a little cavity of rock that looked like it, but it was so fine he could never be sure. He had it somewhere and was always going to locate it for me, but I never saw it.

ASPATURIAN: What, if any, outside consulting did you do during this period?

LOWENSTAM: I started off, actually, before I came here, with Chevron Research. Allen Riley, who became a director for them, had helped me so much that I felt I owed it to him to help him. Shell Oil Company Research was also very intriguing, because there were very good people there. So I consulted regularly there, and with Chevron.

ASPATURIAN: How about the natural history museums here in Los Angeles? Did you have any relationship with them?

LOWENSTAM: I've been an associate curator with the LA County since I retired. I've been associated with the Page Museum, too. As a matter of fact, I prepared a paper, which still hasn't been published, on some shrews that have teeth capped by an iron mineral. The Page Museum is very interesting. But the other one is very good, too. It's now becoming a major paleontology center here.

Overall I haven't done very much consulting, compared to others. One of the best students I ever had—Bob [Robert N.] Ginsburg—went to Shell Research. He was so good that when he insisted on having his own lab in Coral Gables, to pursue his studies on recent marine sediments in relation to oil accumulation, they gave it to him. He and I have done quite a bit of work together in the Caribbean—Jamaica and the Grand Bahama Banks.

ASPATURIAN: How much time out of an average year were you away doing field work?

LOWENSTAM: I would say about three months out of the year.

ASPATURIAN: And you taught as well? Did you have a full teaching load?

LOWENSTAM: I taught two and a half courses, but I always arranged that they would be from January to June. That way I had summer and fall free. Sometimes I spent much more time away than three months.

ASPATURIAN: I wanted to ask you about continental drift theory. When it was finally vindicated, what was the reaction of your colleagues? Had any of them been proponents of the theory before that?

LOWENSTAM: No one, except for Beno Gutenberg. He was really the proponent. He was one of the few in this country who dared to talk about it as a valid idea. After it was demonstrated to be true, I think a number of people here felt like fools that they hadn't accepted it earlier, because it was Ken Caster at the University of Cincinnati who had done a lot of work in South America and South Africa and always pushed the thing. At meetings, whenever somebody tried to explain something in different terms, he would get up and and say, "But the logical interpretation is continental drift."

ASPATURIAN: Had he been here on the faculty?

LOWENSTAM: No. They knew he would come up with that, and they always laughed at him. Afterward, like with most ideas, they accepted it and then forgot they were ever opposed to it. [Laughter] It's so common.

ASPATURIAN: You mentioned that while you were a student at Chicago—and afterward, when you came here to Caltech—that your own interests in marine ecology were not shared by most people. When did the situation change? When did it become a respectable field in itself?

LOWENSTAM: Paleoecology, marine ecology? In biology, they paid no attention to it. But when

Wheeler North, whom I have known since he was a student at La Jolla, came here in environmental engineering [1963], that was a major step forward. As a matter of fact, for at least two decades the environmental engineering PhD students were required to take my courses and I was on their PhD committees. Jim [James J.] Morgan insisted. He was the one who really pushed it.

ASPATURIAN: In addition to the geobiology, what courses did you teach primarily?

LOWENSTAM: I taught a regular two-term paleontology course. It was invertebrate paleontology, but it covered ecology, animal geography, evolution, and geochemistry at the same time. Lots of biology students took it. One became my PhD student and worked on chiton teeth, and another one wanted to get his PhD with me. When that happened, the biologists got upset and didn't recommend my courses anymore—neither geobiology nor paleontology.

ASPATURIAN: What effect did sitting at the intersection of two or three different disciplines have on your research or career here?

LOWENSTAM: In biology, George Beadle was very interested in seeing what I was doing. He realized that I was looking for some genetic traces in biogeochemistry. Max Delbrück was interested up to a certain point. First more as a joke, and later he became quite interested in it. And environmental engineering put emphasis on ecology. Ecology was over there, not over here in geology. So in those days, I often worked with the environmental engineers and the biologists. I took advantage of the interdisciplinary aspects.

HEINZ LOWENSTAM

SESSION 7

July 26, 1988

Begin Tape 7, Side 1

ASPATURIAN: I wanted to ask you today how you got interested in biomineralization and to talk about your research on that here at Caltech.

LOWENSTAM: It basically goes back to the beginning of what I set out to do with fossils—to get the most possible information one may get from what is preserved. I just realized, too, this week, that I started to look at the fossils I particularly like when I was a high school boy. I still have them, and I have built up the collection. In the last five years we were several times in Italy, where I collected more, to complement the upper Silesia forms. They're crinoids—very beautiful sea lilies. I'm going to write a paper finally, on them.

ASPATURIAN: For the first time, on this subject?

LOWENSTAM: Yes. And I think I can do so much more now, coming back to biomineralization per se. In a way, I had also started very early in my career to look at carbonate skeletons. Originally I had also looked at calcite and aragonite skeletons—like shells and various other sea urchin shells, or what have you. And I had started to look at the trace-element chemistry I told you about and the stable isotopes, thanks to Urey. The trace-element chemistry was thanks to Rankama. So when I came here, in 1952, I continued my first work, and I cooperated with Sam Epstein. We wrote a number of papers. One, in particular, was fun because Sam at first didn't believe that there was anything to this particular example. How it started was that one day a representative of Texaco Research came in and put a little vial on my table and said, "This contains some crystal needles, called sedimentary aragonite needles, from Grand Bahama Banks, which is covered with them. You might like to think about what this is. Not do any work, but if you happen to look at them sometime, maybe you'll get some ideas how they originated. We gave it to four other people the same way, and maybe somebody comes up with some answers." Well, I finally found out what it was, but the way I did it shows you my idea of the scientific

method. For quite some time I had been studying algae calcification, because a specialist on recent marine algae in Bermuda had told me that as a geologist I ought to pay more attention to it. I took him at his word, and so he took me around, and I collected all the different forms. We looked at the mineralogy, which was just still calcite and aragonite. And then I got the idea: Let's do a study of the trace-element chemistry; maybe we can distinguish between the green, red, and brown algae. In the fossil records they talk as if they could be easily distinguished, but I didn't believe a word, since they were "matching postage stamps." The attitude was, if an ancient fossil resembled the present-day form, 600 million years ago it was the same group. I wanted a little bit more of a platform to stand on firmly. So I worked, and nothing looked very interesting. The way I prepared the samples for study was to throw them into Clorox. The organic material would oxidize and the sediment would be the crystals, and then you would analyze their trace-element chemistry. Well, one day after two months of working on it, I happened to focus on the fact that when I threw a sample into the beaker with Clorox, the thing just exploded into a dust, clouded the fluid. For the first time, after two months, I focused on what that might mean. In those days, we didn't have scanning electron microscopy. I washed the sample in a hurry and put it under the petrographic microscope, and I saw needle-shaped structures. And then I remembered that vial on my desk. I prepared a smear slide of it—I'd never looked at it under a microscope before. Fortunately I labeled the two slides—the algae and the needles—because it turned out you couldn't distinguish between them. So I got the idea that these sedimentary aragonite needles were derived from a whole group of algae that had a very loose skeleton and post mortem disaggregated into these fine crystals, which people had assumed was inorganic precipitation from sea water.

ASPATURIAN: But your theory was that it was organic—from the algae.

LOWENSTAM: Yes. So I published a first note in the *Journal of Sedimentary Petrology*, around 1956. I only discussed the algae that I had found had these needlelike structures. I said that that suggested an algae origin throughout the Bahama Banks. Then I got to thinking more about it, and I thought, How can we further establish whether or not that material, which accumulates on Grand Bahama Banks, actually is that stuff? So I went out with one of my former students, Bob Ginsburg—whom I mentioned before—who was working with Shell Research in Miami. We

would take a seaplane, fly around and land on the water, take samples, and then go back. I got quite good coverage of that material.

I got the idea that one way to test what the Grand Bahama Banks material was would be to compare the oxygen-carbon isotopic composition of those sedimentary needles from Texaco Research with algae needles on the Banks, and also compare it with oolites, which are little ball-bearing-like structures. They are little balls of calcium carbonate that form on Grand Bahama Banks in certain areas and were considered inorganic in origin. But Sam Epstein said, "No, no. Doesn't make sense. What would you prove?" So I started to prepare the things—and here is an interesting sidelight of Caltech. I needed a new technical assistant for the project, and one day this lady came in and applied for the job. She was dressed up to the hilt, like a native from Paris mode. And Bill Otto, an assistant who was here with the former chairman [Chester Stock], shook his head when she came in. So I made the position sound very difficult, describing the tedious job and all these things. She finally blew up and said, "Do you mean to tell me you don't want to let me try it?" I said, "Certainly, if that's what you want." She turned out to be one of the best people I ever had. It turned out that she was the wife of a visiting professor who was here studying desert ecology—a visitor from Argentina. She was a corporation lawyer and was just bored to death sitting around. So she was the one who prepared the samples for this study.

Well, things started to look very interesting. And Sam came in one day and looked at it, and said, "Jesus, real interesting!" And the next week he came in and looked at the samples and the results I got on this machine downstairs, and he said, "Do you mind if we do a joint study on it?" I said, "No. That's fine." [Laughter] I was just laughing.

So, sure enough, we got out a paper that basically made a very strong case that these crystalline needles were derived from algae [Lowenstam, H. A., and S. Epstein, "On the Origin of Sedimentary Aragonite Needles of the Great Bahama Bank," *J. Geol*. 65(4): 364-375 (1957)]. That seemed to solve the whole problem. But then a friend of mine from USGS—the United States Geological Survey—Preston Cloud, made a study of the same material, just physically, and came to the conclusion that it was chemical precipitation from the sea, and he played around with our temperature data to make them fit this idea. A lot of people accepted that for a while, until one of my former postdoctoral fellows, Lynton Land, made a study of Little Bahama Banks. Preston Cloud's argument had been that he couldn't see enough algae on the Grand Bahama Banks to account for this amount of needles—which, offhand, made sense. However, I never

responded to it. I saw in Bermuda that there was something peculiar going on with this algae. There are certain species that only come up from a dormant stage—from a rhizome—in midsummer, for about two or three weeks. On coral reefs, the whole area is often covered with these species and they have these needles. Then they die, and all the needles, of course, go out into the water as sediment. That would account for the quantity. So I thought, I'm not going to argue. Then Lynton and one of his former students—he's at the University of Texas—made a study of Little Bahama Banks and found that the sedimentary needles—which were very abundant there, too—were not enough to account for all the algae needles produced on the Banks and that most of them even went over the edge of the ridge of the Banks and out. And after that, people started to wonder. I think today, if people are inclined to think in inorganic terms, they will be inclined to think in Preston Cloud's terms; and those who are biologically inclined usually—from what I see in papers—use our interpretation. I think we are right, because I have collected more data since—never published—that really nails it down. But I got my answer.

I often do that. I don't publish everything; I find data just to satisfy my own curiosity. So that brought us up to a certain stage. But my main interest was always carbonate. I didn't even study phosphates, which are the primary material of bone and teeth in all the vertebrates, from shark teeth to man. Eighty million years ago there were even still some birds that had teeth—their inheritance from the reptiles. The medical people supported a lot of research through NIH [National Institutes of Health] in bone studies and bone minerals. So it was either study phosphates or be a shell collector—the category I put myself in when people asked me what I was: "Professional beachcomber." [Laughter] We studied carbonates. I did nothing but that, and we got lots of literature started through that from the Chicago days, through my former students.

One day when I was in Bermuda, about the time when I collected these algae, I was feeling a little lazy, so I went out and sat in the intertidal zone, on the platform. Randolph Taylor, the famous algologist from Michigan, had made me look at it. I was always lucky—I had people who made me look at things, and he was one of them. I had an awful time getting out there, because—apparently from sea spray, although that has never been fully explained—the whole calcium carbonate platform, limestone platform, is as rugged as it can be in its microtopography. Walking is torturous. I finally got tired, and I sat down next to a tide pool, and I noticed that the bottom of the pool was perfectly flat. So I remembered my friend Ken

Emery at USC, who had made me go with him out on the *Velero*—the USC research ship that studied offshore organisms in deeper water. He had written a paper saying that he had seen this type of flat bottom in the tropics, in Guam, in the South Pacific. He thought that they were due to the fact that at night not only the animals there but also the algae would respire carbon dioxide, which would enrich the water in CO_2, and that would tend to dissolve carbonate. So the tide pool bottom was smoothed out by that. And while I thought how right he was, I noticed that there were at the bottom funny chevron-shaped markings. And while I was puzzling over those, a chiton came by and started to make fresh chevrons. And I saw that they were clearly indented into the limestone, which meant that the teeth of these beasts had to be harder than limestone.

That was the beginning of the whole story. I rushed to the library, and fortunately, a friend of mine, Fritz Haas, who was originally from the Senckenberg Museum in Frankfurt, had donated a whole set of [H. G.] Bronn's *Klassen und Ordnungen des Thier-Reichs* to the biological station. Everything to be known about animals was in that compendium, and it was brought up to date every 50 or 100 years. I looked up what chiton teeth are made of, and it said of a material like insects' wings, which is soft and couldn't scratch limestone, calcite. So I knew something was wrong. I took a bunch of chitons and I pulled out their teeth, and they were black, and I still didn't catch on. We brought the teeth back; we obtained X-ray diffraction patterns to determine the mineral. But being accustomed to limestones and phosphates, I used copper radiation, and in copper radiation your image gets fogged if the material you're studying contains iron minerals. And I didn't know that. Still, when I didn't get anything, I talked to Sten Samson, who retired recently in geochemistry. He's very good at X-ray diffraction. He looked at it, and he said, "Whatever the mineral is, it's radiating from the film, so you can't see what the diffraction pattern is. You need to use cobalt or nickel radiation." So we did, and we got a perfect pattern. There's a big index for these patterns. We looked up the patterns of everything we thought it might be, and we couldn't find what it was. Finally, one day someone said to me, "Why don't you go systematically through them until you find one that matches?" And, sure enough, some research assistant said, "Looks like it fits magnetite." I looked again, and I said, "Oh, come on." We went down and we compared the spacing, and we measured everything. It was magnetite. Now, I had studied under Professor N. L. Bowen, the famous man who synthesized minerals from the magma, and so I knew that what you call the P-T—the pressure-temperature—diagram of magnetite was biologically "out of this world." It was formed

inorganically at high pressures and high temperatures down there, not up here at the surface. So, I just wondered.

About this time, Julian Goldsmith, a friend of mine from the University of Chicago, came to UCLA with a famous mineralogist. Julian studied magnetite, various properties of it, from ore deposits, so I went over to UCLA with the X-ray diffraction film. The other fellow with Julie was very funny. He took the X-ray pattern, looked at it, and said, "Magnetite. Big deal, so what?" I said, "It comes from an animal. It's from an animal." He looked at me, and he said, "Yeah, sure." I said, "It is. I took it out of the mouth of the animal." He said, "Maybe he chewed some magnetite for dinner one day." I said, "They all have it." "Oh," he said, "Go away." He never believed it. Julie looked at it and said, "It's magnetite and that's all I can say, but I don't believe animals formed it. I don't know how they got it." But I finally convinced myself it was made by the animal. I carefully removed and dissected the tongue plate, where the chiton teeth are mounted. You could see the whole development—how it was being mineralized. I was quite sure on the basis of that that it came from inside the animal and couldn't be extraneous material.

I also sent a note to Bob Sharp, telling him what I found. I shouldn't say this—the present chairman [G. J. Wasserburg] would never have helped me with this. [Laughter] Because it was biology and not geology.

ASPATURIAN: How did Bob Sharp react?

LOWENSTAM: Bob said, "It's great. You have to publish it fast." I said, "How?" He said, "I have some pull with the Geological Society of America. Get a note together with some illustrations. I'll push it through." And within four months, in a publication where it usually took two years or more, this thing was published ["Magnetite in Denticle Capping in Recent Chitons (Polyplacophora)," *Geol. Soc. Amer. Bull*. 73(4): 435&, (1962)].

ASPATURIAN: Do you remember the year?

LOWENSTAM: It was '62. Oh, I had resistance all over. Nobody believed it.

ASPATURIAN: In the entire geology community, here and outside?

LOWENSTAM: Right. I talked earlier about Al Engel. When this came out, he invited me to La Jolla to give a talk on it. He thought he was going to have a big joke. I had just showed the first pictures of it when he got up and said, "Professor So-and-so at the University of Missouri told me that little beasts like forams can select any kind of mineral and make a coat out of it, like rutile crystals, which is very rare"—which is true. "How do you know that beast you are dealing with isn't just a magnetite collector?" I said, "Al, why don't you sit down and I'll show you a few more pictures, and then we can discuss it." So I showed them what I had found out and why I had published the original paper. I had a graduate student then from biology—Mike [Michael H.] Nesson—who was doing his PhD with me. He had found that when you dissect carefully the radula sheath you find that the tissues that are responsible for the formation of the teeth—the source, the pipelines—are tightly adhering to the teeth. They envelop it so tightly that you have to tear them off. They're connected with the organic matrix of the crystals until the mineralization is completed. And then the curtain lifts, and the teeth are ready for use and exposed to the external environment. And that's a big trick, too, how they make it. The reason they can make a mineral in an environment where it shouldn't be formed is because it's all internal.

ASPATURIAN: So there was no way they could have extracted it from an external source.

LOWENSTAM: Right. After that, he never got up again. [Laughter] That was the second lecture where I got Al. [Laughter]

That discovery really got me thinking: If a common intertidal organism, worldwide in distribution, can form some mineral that nobody had ever suspected or looked for, the process must not be that rare. There must have been lots of naturalists who had seen other animals scraping with hard teeth. Within the same year, I published another paper on limpets, showing that their teeth are made up of another iron oxide mineral called goethite ["Goethite in Radular Teeth of Recent Marine Gastropods," *Science* 137 (3526): 279& (1962)], named for the poet Goethe, because he was also mining director for the Count of Gotha. That's how he made a living. [Laughter] And opal. So here I already had three new minerals. Opal was known to be

formed from sponges and diatoms but not from any higher invertebrates. They were not supposed to make use of it. I got to wondering, What else does life produce and what is the impact of these processes on the biosphere and the evolution of the biosphere, as these organisms evolved? And that's how I got interested in it, by sitting idly on a tide pool.

ASPATURIAN: Was Al Engel's reaction typical of the reception this work got?

LOWENSTAM: Oh, yes. For years after that, they made jokes about it. It took about twelve years to be accepted. I published in 1962. It wasn't until [Richard P.] Blakemore published his paper on magnetotactic bacteria in 1975 that people began to believe it ["Magnetotactic Bacteria," *Science* 190: 377-379 (1975)]. I was called in by the big shots at Woods Hole, where he was a postdoc, to look at the young man's stuff and see if it made any sense. He had discovered some bacteria that were drawn to a magnet if he put it on the petri dish, and that would swim in the opposite direction if he reversed the magnet. So his supervisors asked me whether what he had done was sound work. They didn't know yet what was causing it. I got all excited about it, and I suggested they should get X-ray diffraction determinations. They did, and it was confirmed. It was magnetite.

ASPATURIAN: Was this the first independent confirmation of your work?

LOWENSTAM: Yes.

ASPATURIAN: So it took more than a decade for this to be taken seriously.

LOWENSTAM: Yes. You know, Delbrück for a long time was one of those who couldn't take it seriously. He didn't think it was important biology. Then, when the news came out about the magnetic sense in bacteria, he changed his mind. I remember him saying to me, "I was wrong. This really is important. It does mean something."

It used to be the same with my trace-element work—with my early carbonate minerals work. There was a lag before acceptance, usually from ten to fifteen years. And by the time they were all running down that avenue, I was doing something else.

ASPATURIAN: What about the discovery of the ancient protein material?

LOWENSTAM: Oh, that was interesting, too. When we worked in Urey's lab in Chicago, I brought in possibly the best preserved fossils there were, in order to get a minimum or no effect on the isotopic composition due to diagenetic processes and so on. Those fossils usually gave us trouble, because they had organic material in them. The people who extracted it were usually swearing about it, because the process of freeing that organic material from the skeleton carbonate caused the fractionation of the oxygen isotopes, so it couldn't be used. This was a big problem until Sam [Epstein] developed a helium roasting procedure to get rid of the organic materials. So to us it was a real nuisance.

I remembered that problem and thought about it again, after I looked at the biomineralization products for a while. I said, "After all, in most cases, the skeletons or hard parts are just like a brick wall. The minerals are the bricks, and there's an organic mortar cement around it. What does it consist of? What's its function?" I had had organic chemistry way back in Munich, but I wasn't going to go out on a total new thing. But it always bothered me. Then one day, Ed [Peter Edgar] Hare, a graduate student who had got his master's degree in organic chemistry, came in. He came in when I had a student, Bob [James Robert] Dodd, who was studying blue mussel shells—looking at the effect of temperature on the trace-element chemistry and composition of calcite and aragonite—the two carbonate mineralizations that I had suggested were affected in this group by the temperature. I had done just a brief survey of it, and Bob used it for a doctor's dissertation that focused in great detail on the trace-element chemistry and trace-element composition of these minerals and stable isotopes. About this time, Ed came in and said he wanted to have something to do for his PhD work. I said, "But you are a chemist." He said, "Oh, there must be some organic material around." Then I remembered and I told him the whole story. I said, "Ed, why don't you take the same material Bob Dodd used. He works on the minerals, and you will look at the organic matrix."

ASPATURIAN: Were you his adviser?

LOWENSTAM: Yes.

ASPATURIAN: What did he get his degree in?

LOWENSTAM: Geology, because we had no geobiology in those days. It was in the late fifties and he got his PhD in the early sixties. I got [Henry] Borsook, who was over in biology, to be on his committee. So Ed Hare did the first amino-acid determination ever made on the glycoproteins in shells, using an amino-acid analyzer instead of a qualitative approach. It was the first quantitative determination of the amino-acid composition of organic matrices. He got some fantastic results. They were totally new, because he used the shell calcite and aragonite, and they have organic matrices in which the amino-acid compositions are slightly different. He got very excited about that. I'll never forget what Borsook said at Ed's PhD exam. Before Ed came in, he said to the geologists, "You know, this young man has found something we never thought was there. We thought all of these proteins were alike, and here he shows that there's partial relation to minerals. To tell you why I think he did an outstanding job—whenever I used particular demonstration materials, I would ask Ed Hare to come over and prepare it for me; then I could demonstrate for the classroom without any trouble." Ed is a first-rate chemist. He's at the Carnegie Institution in Washington. He was hired by Phil Abelson, the former editor of *Science*, who was the director of Carnegie's geophysical lab at the time.

ASPATURIAN: How did your colleagues react to the interdisciplinary direction your work, and your students' work, took?

LOWENSTAM: In a way, they didn't understand it often. In another way, there was an uneasiness about it. And in a third way, they saw that my students all got jobs in a hurry. [Laughter] Abelson hired Ed before he got his PhD. So that was the beginning.

Then, not much happened with it for a long time. People did amino-acid determinations, and I got very unhappy about it after a while, because they were always taking things apart and nobody put anything together in molecular structure terms. And so, in '72, when I was at the Hebrew University of Jerusalem, my last seminar was entitled "What We Ought To Do Next, and Do Pronto." Two-thirds of the talk was devoted to pointing out that Ed Hare had opened new avenues when he showed that one can distinguish organic matrices by amino-acid

composition in living organisms and in fossils. I said that by this time we knew how to cleave at specific sites, which Ed couldn't do at the time he was getting his PhD. That meant we might now be able to get amino-acid sequences and determine the molecular structure of these things. [Laughter] I'll never forget this. People were as unimpressed and bored as they could be. Some were asleep; some looked at their watches; others were yawning. I went home, and I said, "Well, I really blew it today."

Begin Tape 7, Side 2

LOWENSTAM: The next day, Steve [Stephen] Weiner, a student who had just gotten his master's degree at the Hebrew University, came into my office and said, "You know, that one thing you talked about yesterday sounds very interesting. I have no real background in organic chemistry." He said he had got his bachelor's degree in Capetown—he was originally South African. "I had a little organic chemistry in Capetown; I have done nothing since I came to the Hebrew University. Do you think you might take a chance and let me try to take a look at that question you raised?" I said, "It's free for all. That's why I'm talking about it." So he came here to Caltech, and I talked to Lee [Leroy E.] Hood. And he was willing, at the beginning, to teach him the sequencing techniques and to have his assistant teach him the techniques. Steve had to take biochemistry courses, and he took Lee Hood's courses. He did everything with flying colors. Pretty soon, he got started. He studied the blue mussel, which we get here all along the coast. That's what Bob Dodd had worked on, and Ed Hare. Steve came back to the same, and to some other groups of shells—mollusks, clams. Then he started to try to sequence this stuff. To everybody's amazement, it turned out to be a very simple repeat sequence, where they had once expected a long, very complex arrangement. Then Lee Hood got interested, too, and supported everything. So Steve got his degree. And that opened a totally new alley. In a way, I am very pleased about that part—that I have been responsible for the first student to get the quantitative amino-acid composition and the first one who put the pieces together. This is now going into highly sophisticated operations—immunology and all kinds of applications, you name it. Steve now has a nice group at Weizmann Institute. I have been going there since '79, every other year. We did some work together, and we have a book coming out [Lowenstam and Weiner, *On Biomineralization* (Oxford: Oxford University press, 1989)].

ASPATURIAN: I think since the mid-seventies the whole biomineralization field has really exploded. Do you feel it's going in the right direction?

LOWENSTAM: I'm pleased with one thing: that I myself, with the help of my former students, Steve in particular, have gotten people who work on pathological questions of bone and teeth to look at their work in the broader context of biomineralization. I've tried to show them that their work is only a small part of the whole biomineralization process, and that if one looks at the broader picture and gets information on how these processes work, one might get feedback in terms of better understanding what is going on in normal processes and maybe pathological ones, too. I myself have worked a little bit on necrotic things. I had George Rossman helping me. The pathologists from Huntington Hospital came in one day, when, if I remember correctly, Barclay Kamb was chairman [of the geology division]. They had looked at some myocardial sections of a patient who had died, and saw some crystals, and they wondered if somebody could help determine what they were and what they might tell about the cause of death of the person. So George and I both worked on it, and we published a paper in the *Archives of Pathology* [Lewis, R. D., H. A. Lowenstam, and G. R. Rossman, "Oxalate Necrosis and Crystalline Myocarditis—Case Report with Postmortem and Crystallographic Studies," *Arch. Pathol*., 98(3): 149-155 (1974)]. George did the mineral determinations, because he had a very small quantity of material for infrared spectroscopy. There was very little in the heart, because they had thrown most of it away. Then I started to look at what other organs they had left, and I discovered that the thyroid gland also had mineralization, only a different kind. So did the kidney—that's to be expected. I did a lot of scanning electromicroscopy, and recently a new atlas came out in Hungary on pathologically formed minerals. They took all my data and put them in there. So this work also, I'm sure, has an application to pathology and, with the junk foods we get today, to all the dangers we might avoid in the future.

ASPATURIAN: You were elected to the National Academy of Sciences in 1980. Was this a big honor for you?

LOWENSTAM: I'll tell you something that I don't want to have published now or shown in any form. In 1954, Beno Gutenberg, Hugo Benioff, and I had lunch, and Beno said, "We want to

elect you and Harrison Brown to the academy. What do you think? Do you want to be this year or next year?" I said, "Well, why don't you take Harrison first, because he is head of the geochemistry section. It might be helpful to get money and other things for his research. And I'll wait till next year." Well, what happened in the meantime, between the two years, was that geology and geophysics were separated. The following year, they couldn't elect me, because they could no longer nominate me. The other people who easily could have elected me in the following years—I didn't like their personalities. They were too self-centered, and I wasn't going to be friendly to them just to be elected to the academy. I was known in that group for being very aloof. So it took thirty years. When I was elected, I was even wondering if I should not accept it, or should do what Dick Feynman did—accept it and resign. But finally I said, "That's childish." Still, I don't go to the meetings.

To me, that kind of elitism was nothing really new, because it started with the Geological Society of America. GSA was very exclusive, all-American, and they had no Jews. So when Bryan Patterson first said I should apply and I did, I wasn't elected. That went on for several years. Later, when I became better known, they tried to induce me to apply again, and I wouldn't. Then one day Bryan came and said he was on the GSA council at this time and it would make it easier for him if I would accept. [Laughter] They elected me very fast and made me a fellow in a very short time. So, to me, that was nothing new. [Laughter]

HEINZ LOWENSTAM

SESSION 8

August 2, 1988

Begin Tape 8, Side 1

ASPATURIAN: You went back to Germany for the first time in 1981. What were the circumstances leading up to that?

LOWENSTAM: I was invited to come to Munich to get an honorary doctor's degree.

ASPATURIAN: The one that had been denied you in 1937?

LOWENSTAM: No. They said that had nothing to do with it. It was strictly on the basis of my research, particularly on the fossil reefs. I don't quite believe it. The thing started very funny. One day I got a letter from the biologist Lynn Margulis saying that Professor Cohen at MIT, who was a friend of the president of the University of Munich, was in correspondence with him and had received a letter to the effect that they wanted to give me an honorary doctor's degree but since I was well known never to answer any German letters or attend any German meetings, they were dubious that I would accept it. They didn't want to be turned down; they didn't want to lose face. It turned out that the recommendation to give me an honorary doctor's degree had come from a number of young people who were born after the war. One of them was Wolfie [Wolfgang E.] Krumbein, who was a young professor at the University of Oldenburg. He had been present at the first NASA microbiology seminar, in which he and I both taught sections. Lynn had introduced me at the meeting by saying that I didn't get a doctor's degree in Germany but I got one in Chicago and that was good enough, or something to that effect. He was a graduate of Munich, and he inquired and found out what had happened. Apparently he started this with several others, including one of my fellow students at Munich, whom I mentioned earlier. We had been students together, and he always was very nice to me. He had something to do with it, and I think, in the end, Lynn Margulis was involved with it, too. I was against going and had many debates.

ASPATURIAN: With whom were you debating?

LOWENSTAM: At dinner parties, with friends. And people with parents or other relatives who were from Germany said no, and the others said, "Sure, you should go." So finally, it came down to should I refuse and insult the young people who were responsible for it, or should I go. So I decided I'd go. I asked Steve Weiner, my former student, to come from Israel to go with me, to shield me. They had an international symposium arranged for me, and I had to give a talk in that. The president of the University of Munich gave first a speech, and he said in plain words how terrible it was what had happened and how grateful they were that I did come back. The scroll they gave me said everything but that it had anything to do with my not getting the degree originally. The beautiful case the scroll was in—I remember my son looking at it and saying, "Well, I like the case." [Laughter]

ASPATURIAN: Did you see any of your former colleagues or teachers there?

LOWENSTAM: No.

ASPATURIAN: Broili and Dacqué?

LOWENSTAM: They were both dead.

ASPATURIAN: Had you been in communication with them since leaving the country?

LOWENSTAM: No. I was very bad. After the war ended and we learned about the Holocaust, I simply wouldn't have anything to do with anyone still in Germany. I felt sorry later; I should have contacted certainly Dacqué, and I should have also contacted Assmann before he died, the man who was responsible early in my development for my decision to go into the field. And I never did. It's too bad.

ASPATURIAN: Were there younger colleagues, though, whom you had a chance to see again?

LOWENSTAM: The only one from the old crowd who was there was the former assistant of Broili in my day. He had been in Australia as a prisoner of war. He had been on a scientific research trip there, and was caught without papers at the outbreak of the war, so they interned him. And he became the chairman of the paleontology department at Munich afterwards. But by the time I came, he had retired, too. In Europe in those days, the assistants often were about the same age as their professors. Also, in attendance was this former student friend of mine, who had always taken me away when the Nazi newspapers published particularly bad anti-Semitic stories.

ASPATURIAN: What had happened to him during the war?

LOWENSTAM: I don't think he was in the war. He was director of some water research operation. And the rest I didn't know. It was very weird. I was completely like shell-shocked throughout the whole experience. I think it was in the Rathaus that they held a big dinner for me in the basement, the Keller restaurant. I knew it from way back. It was—I don't know. I didn't enjoy it at all.

ASPATURIAN: How long were you there?

LOWENSTAM: Two days. And then Krumbein had invited me to come with him to the University of Oldenburg to give talks in the department.

ASPATURIAN: You spoke in German?

LOWENSTAM: No. Not in the symposium either. But very funny, there was a Professor [Adolf] Seilacher from Tübingen there, whom I had met before. He had told all the German participants they must talk in German, and then he gave a talk in English.

ASPATURIAN: Do you feel, looking back, particularly in going to the symposium, that you did the right thing? If you had the chance to do it over again, would you do it?

LOWENSTAM: I don't think so. But that doesn't entirely have to do with this alone. Something

else happened a few years later. First of all, Professor Seilacher had told me that I was absolutely welcome anytime to let him know if I wanted to come to Tübingen and give some lectures and be a visiting professor. And I never took him up on it. Then in '82, I think it was, I met two young German scientists at an international conference on biomineralization in Holland. I was at that time a visiting professor in the department of exobiology at the Catholic University of Nijmegen in Holland. [Laughter] I never understood that. It's the only such department in the world. At any rate, I came to the meeting and they said they wanted me, a month or two later, to come and give a couple of lectures at Tübingen. So I said OK. I didn't think; they were kids, so I said OK. Then suddenly I got in the mail a first-class railroad ticket and the date and everything was settled. And suddenly I focused on it, and I thought, "Oh, god!" Not only that, but there was a note with it that I would be staying in the house of one of them. Well, he turned out to be the son of a very outspoken biographer of many famous German and other scientists. He was a member of some kind of group that the Nazis didn't like, and he was even in prison a couple of times. So it was all right. And the other one was a very nice person.

ASPATURIAN: So you went back to Germany.

LOWENSTAM: Yes. I gave the lectures, and we talked about the past and what had happened to the Jews, and his father was participating. I felt it went much better than I ever thought it would. So in 1984, when the biographer's son invited me to come for six months to Tübingen to do research with him, I accepted. And that's when I really became firm about not ever going again. Part of it was going down in the bus from the visiting faculty apartments to the university, seeing all the older people and wondering what they were doing forty years ago. And the other thing was, the young man who was the famous biographer's son turned out to be a real dictator. He had, for instance, research assistants to run the scanning electron microscope who did a fantastic job. And yet he would call them in and talk to them in a way that was incomprehensible to me—like they were idiots. He just demoralized them. And he did a number of other things that were underhanded. I got to the point where I couldn't take it anymore.

ASPATURIAN: Did you cut your visit short?

LOWENSTAM: Yes, a little, because I went to Yugoslavia to collect samples. On the way back, I had to stop to pick up things in Tübingen. I stayed with the other young scientist. Then, of course, the old professor Seilacher was unhappy that I had accepted these young men's invitations when I had never accepted his. He invited me several times to his place in the Schwarzwald for the weekend. He was too Germanic, too, for me. As a matter of fact, he was the last young German U-boat captain. [Laughter]

ASPATURIAN: What an honor! Was that your last trip to Germany?

LOWENSTAM: Yes. That finished it for me. All in all, I would never go back.

ASPATURIAN: Let's go back for a moment, to this department of exobiology at the Catholic University of Nijmegen in Holland. Why did the Church have a department of exobiology? What were you studying exactly?

LOWENSTAM: I was studying bacteria. [Laughter]

ASPATURIAN: And speculating about the possibility of its being found on Mars, or a moon of Saturn, or something like this?

LOWENSTAM: Among other things. But basically I took advantage; I always had the luck to run into Germans. But in this case, Wolfgang Heinen, who was a microbiologist on this team in Holland—an older man—had left Germany after the war. He couldn't take it there. But his wife, I wasn't so sure; she looked like a Hitler Jugend. She was very friendly, but she was also very disturbed. We were together in Badgastein in Austria, looking in the caves—in the hot springs there—for bacteria several times. I only found out later that these tunnels had been done by prisoners because, I think, Goering wanted to hide some of his art works there.

The head of the department of exobiology was an American, Alan Schwartz, the editor of a very interesting journal, *Biosystems*. Lynn Margulis and I had published together on the early evolution of life—on the possible reasons why the early organisms formed skeletons out of calcium carbonate [Lowenstam, H. A., and L. Margulis, "Evolutionary Prerequisites for Early

Phanerozoic Calcareous Skeletons," *Biosystems* 12(1-2): 27-41 (1980)]. I think that's how he got interested in inviting me. All I had to do was give a series of seminars to his graduate students. Another reason he got interested was that I thought we might find DNA or its degradation products in ancient shells and fossils and could get into some genetic evolutionary history, going back. Oh, I had also said that even if there was no life on Mars, one way we might be able to see if the planet had an early organic development similar to Earth's that was cut off—which I didn't believe—was that these early Martian forms might have left minerals similar to the biomineralization products found on Earth. So even if we didn't find any present-day life, if we found a mineral form that shouldn't be on a lifeless planet, that might be indirect evidence for the presence of life at some earlier period. He got quite intrigued by this. I originally had no intention of going there; I had never heard of the University at Nijmegen. It was close to the German border, right next to it. When we went on walks, it was right along the border. That was the famous Arnhem area, where the British parachuted right into the middle of the German armored detachment area and they held it for a short time. It was a big debacle. Lots of Canadians died in that, and they have a big Canadian cemetery there.

ASPATURIAN: You were there for six months?

LOWENSTAM: Yes. That was the time when I went every year to another place. I had been invited the year before to Israel, where I got the chair at the Weizmann Institute for six months. And then to Nijmegen, and then to Tübingen.

ASPATURIAN: What major changes do you think have occurred in Caltech's geology division since you got here?

LOWENSTAM: The changes are primarily that it shifted completely away from basic field geology and field-oriented research. Many of the original PhD theses here were by people who had mapped the area and worked on the rocks right here. That was really the core of the division, outside of seismology. Dr. Sharp was an outsider in terms of the field, but his research clearly fitted into geomorphology. I worked on the fringe, shall we say, but they tolerated me. [Laughter] So, this emphasis on field work, also in terms of courses, is greatly reduced today.

It's on a broader scale and more experimental. Also, with the new developments from our present chairman [G. J. Wasserburg], we've gone into outer space. We had already legitimately acquired the solar system from astronomy and developed the planetary sciences. Now we tackle cosmology.

ASPATURIAN: When the division chairmanship shifted from Dr. Sharp to his successors, were there major changes?

LOWENSTAM: Gene Shoemaker, who came after Bob Sharp, was quite interested in extraterrestrial beings. But he's an excellent field geologist and he always strongly supported the program. We taught a course together—a summer field camp—in the Humboldt Mountains in Nevada. That was great. At that time, he did mostly remnant paleomagnetism and stratigraphic correlation of rocks. So he had a crew that cleaned the section down to bedrock, and he took samples for getting their magnetic orientation. I then went over the same section with my students and had them collect all the fossils from it, which was very nice. The division also went in a major way into planetary sciences, which was very popular at the time, because there were all these space missions. I remember Bob Sharp was involved in doing geomorphology on Mars.

ASPATURIAN: And, of course, there were your six months as an exobiologist.

LOWENSTAM: Right. That was nothing out of the ordinary. [Laughter] Well, under [Peter] Wyllie, things started to drift. The other thing, of course, was that the faculty was getting older. So more young people came in, and all the orientation was away from the old field geology.

ASPATURIAN: Who were the younger faculty you have worked with primarily?

LOWENSTAM: It was mostly in geophysics that they came in. Outside geophysics, there wasn't very much. There was George Rossman. He fits very well into the expanded division here. He's an avid collector of minerals, and we did several studies together.

ASPATURIAN: Joe Kirschvink was an undergraduate here, I believe. Did you work with him at

that time?

LOWENSTAM: I was his undergraduate adviser. I made him take molecular biology in Lee Hood's place. He came along on all my cruises.

ASPATURIAN: Were you a major influence in bringing him back as a faculty member?

LOWENSTAM: I think so. I was also responsible [for the fact] that he didn't get his PhD here.

ASPATURIAN: You sent him away?

LOWENSTAM: Yes. I felt it was time to get him in contact with some new people and new ideas. We even selected Princeton, Gene Shoemaker and I. [Laughter] Gene was a graduate of Princeton, and when we talked about places he said Princeton was good. I said, "Anyplace far enough away from me." But I still advised Joe on his PhD thesis. He did it in Australia. He had decided to go to Australia, but he was going to do something entirely different, and I called his attention to the fact that there was a very famous area there relating to the Cambrian-Precambrian boundary and that it could be very nice to tie the period into the big picture in paleomagnetic terms. And he did.

ASPATURIAN: Do you work with him now?

LOWENSTAM: Not very much at the moment, because I'm trying to finish up a lot of old stuff. But we were in Palau together two months ago in the Caroline Islands, my old hunting grounds in the South Pacific where I got all my samples. That's 500 miles north of New Guinea and 500 miles east of Mindanao in the Philippines. I have been working there since 1950.

ASPATURIAN: What is the current research you're pursuing?

LOWENSTAM: At the moment I'm just finishing up a lot of things I haven't completed before. What's on the back burner is how to recognize in the fossil record types of carbonate minerals

that at this stage nobody even thinks to look for because they are so unstable. I've written a paper on tunicates, which resemble sponges as adults but are really protochordates, our near-ancestral forms. I was looking at a group that hadn't really been studied in detail before—at the shape and mineralogy of the spicules—so that we might look for them in the fossil record. I'm revising the manuscript right now, which has been accepted for publication ["Spicular Morphology and Mineralogy in Some Pyuridae (Ascidiacea)," *B. Mar. Sci*., 45(2): 243-252, (Sept., 1989)]. They wanted it expanded, particularly the illustrations, which is unusual. Usually they tell you the opposite; that's why I was keeping the number down.

ASPATURIAN: Do you take your own photographs?

LOWENSTAM: No. I sit with the people—I never let anybody take them without me. I sit with them, let them work the machine. But I want to concentrate on what I see, instead of all the knobs. So that article should be finished shortly, and I have a short section for a book, which will be published at UCLA, on late Precambrian history. I'm preparing a paper for that on skeletal materials formed by organisms today, which haven't even been reported from the recent marine sediments but ought to be there. It is focused on the Precambrian, where we have a lot of problematica, because people don't know about these present-day forms that may go back to that time. I want them to see what they looked like, and at those meetings at UCLA we had already located a couple of cases of present-day groups going back 570 million years.

The other thing I want to do has to do with something very curious that happened a number of years ago. John Fitch, the research director of California Fish and Game, used to come here to visit, and he got me interested in working with him on shark gravity receptors. They were so tiny that he needed scanning electron microscopy to see them. I got very interesting pictures together for him. One day during this time, he came by and said, "You know, big joke. I have always asked fishermen to give me any rocks they accidentally bring up with their nets or lines in deeper water. And about three years ago I got a big rock with lots of cavities in it. I poked around, and I found little limpet-shaped snail shells—animals—in it. I gave it to a friend in Redwood City, a retired biologist." Then Jim [James H.] McLean, the curator of mollusks at the County Museum here, was preparing a monograph on mollusks—on limpets. He went to John's friend in Redwood City and got these little shells, among other

things, and he tried for years to explain them as limpets. It was two or three years later when John Fitch told me about it. McLean had finally realized that they were representatives of a group of mollusks that were supposed to have been extinct for 350 million years. The Danes had found them alive for the first time in the deep sea in the ’50s. And I had studied them in Denmark with [Henning] Lemche, comparing them with the fossils.

ASPATURIAN: Were these a different subspecies, though, or were they the same ones the Danes had found?

LOWENSTAM: They were a different species, a different genus. Not only that, it was the first time a so-called fossil species had been found in shallow water. All the previous living records, after the first discovery in Denmark, had been in the deep sea. So John said, “They were only 500 or so meters down, at the most.” I got very excited about it, so I borrowed the *Velero*, the ship from USC.

ASPATURIAN: They have their own marine biology ship? Caltech doesn’t have one of those.

LOWENSTAM: We could have had one. I was supposed to get one. Beadle was working on it.

ASPATURIAN: What happened?

LOWENSTAM: I didn’t want to be director of the station, which some trustees were willing to build in Santa Barbara.

ASPATURIAN: No directorship, no ship?

LOWENSTAM: Right. Also, I was against having a ship, because then I would have to use it all the time. I wanted it only when I was ready for it. So, to make the story short, I went out, I got the right equipment. The mollusks were very tiny. I brought them up alive. You see, all the deep-sea forms that had been brought up before were dead by the time they came on shipboard, because of the difference in pressure and temperature. They couldn’t survive the change in

conditions. But I succeeded in bringing them back and keeping them alive up to a year here in the tanks. The most important thing was that I finally found a movie producer who was willing to take microphotos, through a microscope, of the living forms for me. I have the unedited movies here. I want to edit those and use them in part to describe new features of the mollusks I haven't done yet—about their behavior and its implications for the earlier, extinct forms that are now fossils. Also, nobody has ever seen those things running around. So that comes as the next project.

ASPATURIAN: You mentioned that the division's concerns have spread out from the planets to the solar system, and now into cosmology. Is that a good thing in your opinion?

LOWENSTAM: I think it is a very good thing, if it wouldn't be at the expense of other areas. And it looks like it is. Geochemistry is going to suffer. I know that the geochemists are very unhappy because there are no younger replacements for them. If there is a new geochemist, he will be cosmology-oriented instead of toward earth and planetary science. I don't think that is good. Ten years ago, we could have had the best people here in biogeochemistry and organic biogeochemistry. It was pushed by staff members here, but it wasn't accepted at the top of the division.

ASPATURIAN: So they went elsewhere.

LOWENSTAM: Right. That's how we lost Mike [Michael J.] DeNiro, for instance. He was originally one of my students here.

Begin Tape 8, Side 2

LOWENSTAM: He was a student in biology. He worked with Seymour Benzer; and he got bored with those things. He took my course in geobiology and got quite fired up. He wanted to do a doctor's dissertation on certain "living fossils"—the Sclerospongiae—but in Jamaica, where he had to do the work, the lab wasn't a clean lab for contaminants, so he couldn't go through with it. He shifted over to Sam Epstein, then—to nitrogen isotopes and the cycle of nitrogen—and did fantastic work that has great application to our geology. He became, in no time, a well-known

person. He went to UCLA. We could have had him here, and geochemistry wanted him. But then he accepted a job at Santa Barbara.

In a way, Joe [Kirschvink] is alone, too.

ASPATURIAN: Do you keep up with areas that are indirectly related to your field? I'm thinking of some major controversies, like what triggered the end of the Cretaceous, and paleontology versus molecular dating in human origins.

LOWENSTAM: Yes. As a matter of fact, the Israeli anthropologist [Ofer] Bar-Yosef and I did a lot of things together. He recently demonstrated that excavations in Israel showed that *Homo sapiens* lived in the region before Neanderthal man. He just accepted a position at Harvard. I was always interested in archaeology and anthropology. Not dating, really. I consider that more age-merchant business. I like to have the numbers, but as far as that area goes I have a blind spot. To me, it is just to give me some props for doing something else.

About the K-T [Cretaceous-Tertiary] boundary and the controversy over whether [an asteroid impact] ended the Cretaceous, I'm not satisfied yet that we have the answer. What superficially appears to be one big catastrophe may turn out not to have been one big catastrophe but a situation where a few critical props were knocked out and the rest just collapsed as a result. It would be an indirect, rather than a direct, cause. I'd like to see some more independent evidence for certain details. I think Dr. Epstein is getting quite interested in it. We both agree that it couldn't have been caused exclusively by a single catastrophe, as it initially appeared when the iridium layer was first discovered.

ASPATURIAN: Did you ever do any work explicitly on the Cambrian-Precambrian boundary?

LOWENSTAM: I actually worked on that question with Alan Schwartz, the exobiologist in Holland I told you about. You see, there's a tremendous number of skeletal remains dating back to the earliest skeletal organisms, about 550 million years ago. We don't know what groups they belong to, and we need to know to get a picture of the community structure of the early Cambrian. I have an editorial just coming out in the journal *Precambrian Research* that I have entitled, "What, If Anything, Happened at the Precambrian-Cambrian Boundary?" It was

accepted as such. [Laughter]

ASPATURIAN: What is the general argument of that article?

LOWENSTAM: The paper looks at why early organisms started to develop calcium-binding proteins, which are essential to skeletal material, and examines the major role that calcium played in the metabolism of these organisms. The usual interpretations of how the first organisms with hard skeletons emerged don't give any explanation, except to say that when predators came in, organisms got skeletons. Well, your soft-bodied organisms couldn't immediately think, "Calcium, that's armor," and start secreting enough of it to build up skeletons. I felt that it was the other way around. As predators started to come in, there were selection pressures favoring the development of organisms with greater muscular activity and faster nerve responses—the better to escape. And that kind of development would build up calcium further in the organism. It could easily happen in evolution that the calcium buildup could overshoot, and that some organisms started producing more than they could store. So they tried to get rid of it; they tried to extrude it.

ASPATURIAN: It wound up as skeletal material, and that turned out to have an adaptive advantage.

LOWENSTAM: Right. I initially proposed that theory about six or seven years ago. It was very critically looked at, for a while. I think people are coming back to it now that they have lost a number of props in support of other theories.